PHILOSOPHICAL

TRANSACTIONS

OF THE

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FOR THE YEAR MDCCCXL.

PART I.

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MDCCCXL.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body,

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upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

A List of Public Institutions and Individuals, entitled to receive a copy of the Philosophical Transactions of each year, on making application for the same directly or through their respective agents, within five years of the date of publication.

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The Observatory at Wilna.
Professor Bessel, of Königsberg.
The Dépôt de la Marine, Paris.
The Bowden College, United States.
The Waterville College, United States.

ROYAL MEDALS.

HER MAJESTY QUEEN VICTORIA, in restoring the Foundation of the Royal Medals, has been graciously pleased to approve of the following regulations for the award of them:

That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

That the triennial Cycle of subjects be the same as that hitherto in operation: viz.

1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology or Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Physics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose also to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Geology or Mineralogy, communicated to the

Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

The Council propose also to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Chemistry, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

The Council propose to give one of the Royal Medals in the year 1842 for the most important unpublished paper in Astronomy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1839, and prior to the termination of the Session in June 1842.

The Council propose also to give one of the Royal Medals in the year 1842 for the most important unpublished paper in Physiology, including the Natural History of Organized Beings, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1839, and prior to the termination of the Session in June 1842.

The Council propose to give one of the Royal Medals in the year 1843 for the most important unpublished paper in Physics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1840, and prior to the termination of the Session in June 1843.

The Council propose also to give one of the Royal Medals in the year 1843 for the most important unpublished paper in Geology or Mineralogy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1840, and prior to the termination of the Session in June 1843.

Adjudication of the Medals of the Royal Society for the year 1839 by the President and Council.

The Copley Medal to Robert Brown, Esq., D.C.L., F.R.S., for his discoveries, during a series of years, on the subject of Vegetable Impregnation.

The Royal Medal, in the department of Astronomy, to James Ivory, Esq., K.H., F.R.S., for his paper entitled "On the theory of the Astronomical Refractions," published in the Philosophical Transactions for 1838.

The Royal Medal, in the department of Physiology, including the Natural History of Organized Beings, to Martin Barry, M.D., for his papers entitled "Researches in Embryology," published in the Philosophical Transactions for 1838 and 1839.

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PHILOSOPHICAL TRANSACTIONS.

I. *On the Chemical Action of the Rays of the Solar Spectrum on Preparations of Silver and other Substances, both metallic and non-metallic, and on some Photographic Processes.* By Sir JOHN F. W. HERSCHEL, *Bart. K.H. V.P.R.S. &c.*

Received and Read February 20, 1840.

1. LEST the title of this communication should induce an expectation of its containing any regular and systematic series of researches developing definite laws, or pointing to any distinct theory of photographic action, it may be as well to commence it by stating its pretensions to be of a much lower kind, its object being simply to place on record a number of insulated facts and observations respecting the relations both of white light and of the differently refrangible rays to various chemical agents, which have offered themselves to my notice in the course of photographic experiments originating in the announcement of M. DAGUERRE's discovery. The facts themselves, in the present state of our knowledge, will, I believe, be found by no means devoid of interest, and may lead, in the hands of others more favourably situated for such researches, and, I may add, in a better climate than ours, to inquiries of the utmost interest.

2. In a communication to this Society, which was read on the 14th of March, 1839, and of which an abstract will be found in the notices of its proceedings for that sitting, I have stated the circumstances which first directed my attention to this subject, and the progress I had then made, both in the scientific part of the inquiry and in its application to the photographic art. As that paper was (at my own request) withdrawn from the further immediate notice of the Society, and as the abstract alluded to may not fall into the hands of those who may read the present communication, a brief recapitulation of its contents will be necessary to preserve the connexion by which my inquiries have been linked together.

3. The principal points of that communication are as follows. 1st. The use of the liquid hyposulphites for fixing the photographic impression, in virtue of the property which they possess (and which was, I believe, first pointed out in my paper on those

salts in BREWSTER's Edinburgh Philosophical Magazine*) of readily dissolving the chloride and other combinations of silver insoluble in the generality of menstrua. It has since appeared that this is the solvent used by M. DAGUERRE; and I may add that it is the only one that I have found at once easy of management, and perfectly to be relied on under all circumstances, though there are many which occasionally, or with great attention to nicety of application, succeed well.

4. 2ndly. Observation of the feeble susceptibility of the pure chloride of silver to the action of light, and attempts to attain a greater degree of susceptibility, attempts attended then with very limited and partial success, far surpassed by the curious and powerful processes of Mr. TALBOT, not then divulged, as well as by those which I have myself since fallen upon.

5. 3rdly. The application of photography to the copying of drawings and engravings, and to the fixation of optical images. Of course it will be understood that I have no intention here of interfering with Mr. TALBOT's just and long-antecedent claims in this or in any other points; but having bestowed much attention both then and since on these processes, so as to produce results far from unsatisfactory, it will be necessary to say a few words in justification of the time and pains I have devoted to this branch of the inquiry.

6. A susceptibility to the action of light, and a power of destroying at any given moment all susceptibility to further action in the ground of a picture, being granted, it obviously depends only on the degree of that susceptibility, and on the precision with which the rays of light can be concentered on or confined to their intended points of action, how far the photographic impression obtained, whether from the camera or the original drawing or engraving to be copied, shall satisfy the wishes of the artist. As regards the camera; that which I employed was neither periscopic nor achromatic, being in fact no other than the aplanatic crown-glass lens computed and described in paragraph 13 of my paper on the aberrations of lenses†. This combination, though admirable for its original purpose as a burning-glass, is in fact one of the worst possible for a photographic camera, in which the three qualities of a flat field, a sharp focus at great inclinations of the visual ray, and a perfect achromaticity, are indispensable. The latter quality, indeed, (as will be apparent enough from the properties about to be described,) is even more necessary for the photographic than for the ordinary use of the camera. In consequence, my first camera pictures were necessarily extremely defective. Nor was I at all anxious on this head, knowing that the other essentials of the photographic problem being secured, the acquisition of a perfect camera would ensure perfection in that respect.

7. A much more important line of inquiry, as far as applications are concerned, appeared to be the exact reproduction of indefinitely multiplied fac-similes of an original photograph once obtained, by which alone the *publication of originals* could be accomplished. And this seemed the more deserving of attention, as it was under-

* Philosophical Transactions, 1821. † 1819. vol. i. and 1820. vol. ii.

stood that M. DAGUERRE's pictures admitted of no such reproduction, thus giving a decided superiority to the use of paper, or other similar material, provided it could by any means be wrought up to equal perfection of effect.

8. To avoid much circumlocution, it may be allowed me to employ the terms positive and negative, to express respectively, pictures in which the lights and shades are as in nature, or as in the original model, and in which they are the opposite, i. e. light representing shade, and shade light. The terms direct and reversed will also be used to express pictures in which objects appear (as regards right and left) as they do in nature, or in the original, and the contrary. Thus also we may speak of a positive direct, a negative direct, or of a positive reversed or negative reversed picture, to the great saving of words and time. Thus the camera impresses on white paper a negative-reversed photograph. And this being also the character of an engraving transferred on similar white paper, it is evident that a single transfer from a camera picture is equivalent in ultimate effect to a double transfer from an original drawing or from an engraving on such paper.

9. The chief difficulty in the reproduction of drawings or engravings, consists in the necessity of operating a double transfer, so as to produce a resulting impression of the same character as the original in both these respects, whereas from what is above said it appears that this does not apply in the case of a camera picture. Granting therefore that such a picture could be impressed with such depth and sharpness as to be equal in these respects to a finished engraving, the problem of photographic *publication* is solved, since it is a fact that the negative reversed photographic impression of an engraving, however highly finished, such as is obtained by a single transfer on white paper, is quite equal in precision and finish to the original engraving.

10. The above remarks relate only to what may be called the practical part of the photographic art. We come now to points of scientific interest. It is noticed then in the paper above alluded to in abstract, 4thly, That the intensity of chemical action of different rays in the solar spectrum appears to be in great measure disconnected with their colorific impressions on the eye; and this conclusion, generally though unequivocally indicated by a variety of experiments in which coloured glasses were used to analyse and separate the incident rays, will be found confirmed, with many details equally novel and unexpected, in the experiments about to be related, in which the prismatic analysis of the rays has been resorted to.

11. 5thly. A curious property of glass plates in exalting the effect of sunshine on nitrated paper is pointed out; a property of which subsequent experiments have shown the reverse to obtain in certain circumstances, and which the results of these experiments (to be described in the sequel), in place of elucidating, have only served to render more enigmatical.

12. Having no better ground of arrangement under which to group my subsequent experiments, I shall follow this order, so far as it goes, in describing their

principal results, transposing only the second and third heads, so as to keep the scientific part of the subject as distinct as possible from the practical. And first then,

I. *Of fixing Photographs.*

13. As already observed, I find the surest and best fixing material to be a liquid hyposulphite, and of these I prefer that of soda. But in using it some precautions are necessary. The photograph must be first well washed by soaking it in water, to free it from all superabundant nitrate of silver, which is very apt to produce a sulphuret on its first contact with the hyposulphite*. If the paper be prepared with the simple nitrate, the water must be distilled, since the smallest quantity of any muriatic salt present attacks the picture impressed on such paper with singular energy, and speedily obliterates it, unless very dark. A solution containing only a thousandth part of its weight of common salt, suffices to effect this in a few minutes in a picture of considerable strength. But if the paper be already muriated or prepared with other insoluble argentine compounds, this perfect purity of the water is not needed, and a small addition of salt is even beneficial, by removing the silver as fast as abstracted from the paper.

14. This first washing greatly diminishes the sensitiveness of the photograph to further impressions of light, and if merely nitrated, destroys it entirely if the paper be thin. If otherwise, it may be considered as half fixed, and may be preserved, and occasionally inspected in feeble lights, till convenient to fix it completely. To do this it must be thoroughly dried, and then brushed over very quickly with a flat camel-hair brush dipped in a saturated solution of the hyposulphite, first on the face, then on the back. This, having remained on it till the paper is completely penetrated with it, must be washed off with repeated and copious affusions of water, aided by a soft sponge, with a *dabbing* motion, often turning the picture, until the liquid comes off without the slightest *sweetness*. The photograph is then fixed, and may be dried and put by; but to make it quite secure it is best to repeat the process, and if the paper be thick, even a third time.

15. The hyposulphite of soda and silver being liable to spontaneous decomposition, accompanied with separation of silver in the state of sulphuret, it is necessary to be very careful in washing away the very last traces of this salt, especially if it be intended to use the photograph for re-transfers, in which case a deposition of sulphuret within its pores is fatal, since it renders the paper unequally opake. It is for this reason we recommend to apply the hyposulphite concentrated and quickly; since if it be not in excess at every point of the paper, the deposit of sulphuret takes place at the first contact, and can never after be got rid of.

16. Common salt used as a fixing material on paper, when it succeeds, probably acts as a solvent of the chloride or other argentine compound used. At least I find

* See my papers above cited.

that a saturated brine dissolves this substance pretty freely, producing a solution of an intensely bitter metallic taste, from which the greater part, if not the whole chloride, is thrown down by copious addition of water. I cannot say, however, that I have found much satisfaction in the use of salt as a fixing material.

17. Hydriodate of potash, if the right strength be hit, succeeds well; but to do so is not very easy. Moreover, it gives a yellow tint to the ground of the picture, highly unfavourable to transfers. The very curious property possessed by this salt, when applied of a certain strength, of rendering photographic paper, blackened by exposure to light, susceptible of being again whitened by the same agent, has been noticed by Lassaigne, Mr. Talbot and others, and was early encountered by myself. My attempts to apply this property to the production of a *positive* photographic paper, (i. e. a dark ground, on which the action of the light shall impress a *bright* image,) though occasionally successful, and in one or two instances remarkably so, have yet not enabled me to state any precise proportions of ingredients, and mode of applying them, which shall *certainly* produce a paper of the required character possessed of any degree of permanence. In fact, nothing can be more variable and capricious than the results obtained according to the different intensities of the solutions applied—the qualities of the paper—the degree of darkening induced on the paper before the application of the ioduretted solution—the state of the paper as to moisture or dryness, and other circumstances. Nor will this appear strange when the complicated nature of the action itself, when analysed as will be explained in the subsequent part of this paper, is considered. It will there be shown that the total effect of a ray of white light on iodic preparations is in fact the difference of two opposing actions, either of which is susceptible of being exalted or enfeebled at pleasure by circumstances under our command, indeed, in a general way, but difficult to reproduce exactly at our pleasure. When these opposing actions exactly neutralize each other, the paper is insensible. When either preponderates, it is positive or negative in its character according to that of the preponderant action; nay, it may at one and the same moment be positive to light incident under certain circumstances, insensible under others, and negative under a yet different illumination. In reference to practical applications, however, I ought to mention that others appear to have met with better success than I have done, perhaps from operating on a larger scale, in which the proportions of ingredients may be more certainly determined and adhered to. A positive paper of this nature is actually prepared for sale by Mr. Robert Hunt of Devonport, specimens of which he has been so obliging as to send me, and which certainly gives results of great promise in this line.

18. A weak solution of ferrocyanate of potash renders argentine papers insensible to light; and as it is readily procured in the shops, which the hyposulphites are not, it would seem to be a very convenient fixing material. Unfortunately, however, it cannot be used for this purpose, since pictures fixed with it, though perfectly insensible to the strongest sunshine, are gradually but very slowly obliterated in the dark.

The disappearance of the darkened portions goes on with remarkable steadiness of progression from week to week and from month to month, till all is gone and the paper reduced to its original whiteness, or (if the ferrocyanate have been used in excess) to a blue colour.

19. By far the most remarkable fixing process with which I am acquainted, however, consists in washing over the picture with a weak solution of corrosive sublimate, and then laying it for a few moments in water. This at once and completely *obliterates* the picture, reducing it to the state of perfectly white paper, on which the nicest examination (if the process be perfectly executed) can detect no trace, and in which it may be used for any other purpose, as drawing, writing, &c., being completely insensible to light. Nevertheless the picture, though invisible, is only dormant, and may be instantly revived in all its force by merely brushing it over with a solution of a neutral hyposulphite, after which however it remains as insensible as before to the action of light. And thus it may be successively obliterated and revived as often as we please. It hardly requires mention that the property in question furnishes a means of painting in mezzotinto, (i. e. of commencing on black paper and working in the lights,) as also a mode of secret writing and a variety of similar applications.

20. The chromate (and probably also the arseniate) of silver is insensible to light, and in certain cases, therefore, where the colour is of little consequence, might be of use as a fixing material.

21. There is a remark which ought not to be omitted in regard to this part of our subject, viz. that it makes a great difference, in respect of the injury done to a photographic picture by the fixing process, whether that picture have been impressed by the long-continued action of a feeble light, or by the quick and vivid one of a bright sun. Even supposing the pictures originally of equal intensity, the half-tints are much less powerfully corroded or washed out in fixing, in the latter case than in the former. It is probable that other atmospheric relations than those which refer to the extinction of the merely luminous rays are concerned in this phenomenon. In an atmosphere so loaded with coal smoke as that of the neighbourhood of London, peculiarities of absorptive action may have place which rarely or never occur elsewhere. The tint of coal smoke is yellow (as may be seen in perfection in a London November fog), and more than one instance of the intense power and capricious singularities of very pale yellow media in their action on the chemical rays will come hereafter under our notice. In the locality from which this paper is dated, a light easterly wind brings with it abundant smoky haze from London, to which rural prejudices assign the name of "blight" and attribute an insect origin. On such occasions, when the sky has been otherwise cloudless, (comprising nearly half the poor allowance of sunshine of the last summer,) I have been continually at once annoyed and surprised, by the slowness of photographic action, and by the fugitive nature of its results under the process of fixing.

II. *Of taking Photographic copies and transfers.*

22. I shall not long detain the Society with an account of the various trials I have made for perfecting the multiplication of photographs. The following are the chief precautions it will be generally found necessary to attend to.

23. *First.* An extremely close contact of the photographic paper with the original to be copied, which can only be ensured by subjecting both (laid face upon face) to the pressure of powerful screw-clamps acting on plates of very thick glass. The largest pictures I have copied occupied 110 square inches of exposed surface (eleven inches by ten), and for these dimensions a thickness of glass equal to the one third of an inch was found barely sufficient, or rather too weak. This thickness is, however, ample for photographs whose dimensions do not exceed fifty or sixty square inches. The glass should of course be as colourless as possible, and rather verging to a blue than a yellow or pink tint, yellow media being especially active in absorbing the chemical rays, as will hereafter be shown. The glass should press the papers on some soft bed, such as velvet, stretched on a thick board of a truely plane surface. The contact should be as close as can be produced by any pressure short of what will break the glass, as the smallest interval is injurious by allowing dispersed light to spread. Even that arising from the interposition of a thin film of mica is hurtful to the sharpness of the impression.

24. When only a single transfer is to be made, closeness of contact is the only precaution to be attended to; and this being taken, the transfer takes place with perfect fidelity, and with a depth and sharpness fully equal to the original. But if the photograph so obtained be used for making a re-transfer, a host of annoyances and difficulties arise from the imperfections and unequal and uncertain texture of the paper, as well as from the subtle nature and want of absolute opacity in the reduced silver deposited under the influence of the light, and in which the design is traced, to which must be added the great care which must be bestowed on the fixing process and final washing out of every particle of silver except what actually enters into the design, and which is by no means easy if any but very thin paper be used.

25. Such paper is easily procured of very even texture, but if examined in the light it is found penetrated with actual *holes*, which though of minute dimensions, suffer light to pass freely and are very injurious. To obviate this, and to equalize the light by dispersion, an additional thickness of such paper may be advantageously interposed between the glass and the photograph to be re-transferred. And, finding this in fact to be quite necessary to the production of a tolerably good effect, it occurred to me to impress this paper also with a facsimile of the original, or in other words to double the photograph intended to be re-transferred. This may be done by taking off two such photographs from the original design, which being fixed, dried and pressed flat, are to be applied to each other (face to back), and being first delicately adjusted under a magnifier, by fiducial points made with a fine needle in each, are in

that position to be cemented together at the edges, or rather at four points, one in the middle of each side. The operation is easier than it would appear to be by the description, and the resulting effect offers an infinitely nearer approach to that of an ordinary copper-plate engraving or mezzotinto, than anything that can be got from a single model.

III. *Of the Preparation of Photographic Paper.*

26. It would be tedious and serve little purpose, were I to recite individually the numberless combinations I have tried with a view to the increase of sensitiveness and facility of preparation of this useful material; especially as, after all my trials, I am obliged to admit (which I most readily do) that the specimens recently placed in my hands by Mr. Talbot far surpass in respect of sensitiveness any that I have yet produced of a manageable kind, and that for all ordinary purposes I have ended in adopting his process of preparation, as far as I understand it. Nevertheless, as all my earlier inquiries were conducted in ignorance of these processes, and as they have led to some results worth notice even in this line, I shall set down a few particulars.

27. My first attention was directed to the discovery of a liquid, or emulsion, which by a single application, whether by dipping or brushing over, should communicate the desired quality. The presence of organic matter having been considered by some late chemists an essential condition for the blackening of the nitrate of silver, I was induced to try in the first instance a variety of mixtures of such organic soluble compounds as would not precipitate that salt. Failing of any marked success in this line, (with the somewhat problematic exception of the gallic acid and its compounds,) the next idea which occurred was that of introducing organized salts of silver into the pores of paper, by first washing it over with an organic, soluble, precipitable salt, with alkaline base, and then with nitrate of silver. Here also, no distinct result was obtained, except that when the uric acid and alkaline urates* were employed, a paper was obtained decidedly more sensitive than that resulting from the nitrate alone, but which blackened spontaneously in the dark.

28. A great many experiments were made by precipitating organic liquids, both vegetable and animal, with solutions of lead, as also, after adding alum, with alkaline solutions. Both alumina and oxide of lead are well known to have an affinity to many of those fugitive organic compounds which cannot be concentrated by evaporation without injury; an affinity sufficient to carry them down in combination, when precipitated either as hydrates or as insoluble salts. Such precipitates when collected were applied in the state of cream on paper, and when dry were washed with the nitrate. It was here that the first prominently successful result was obtained. The precipitate thrown down from a liquid of this description by lead was found to give a far higher degree of sensitiveness than any I had before obtained; receiving an equal depth of impression, when exposed, in comparison with mere

* From the *Boa Constrictor*. My specimen had been long kept, at least fifteen years.

nitrated paper, in less than a fifth of the time, and moreover acquiring a beautiful ruddy-brown tint almost amounting to crimson, with a peculiarly rich and velvety effect.

29. Alumina, similarly precipitated from the same liquid, gave no such result. Struck by this difference, which manifestly referred itself to the *precipitant*, it now occurred to me to omit the organic matter, (whose necessity I had never before thought of questioning,) and to operate with an alkaline precipitant on a mere aqueous solution of nitrate of lead, so as to produce simply a hydrate of that metal. The result was instructive. A cream of this hydrate, being applied and dried, acquired, when washed with nitrate of silver, a considerable increase of sensitiveness over what the nitrate alone would have given, though less than in the experiment where organized matter was present. The rich crimson hue also, acquired in that case under the action of the light, was not now produced. Two peculiarities of action were thus brought into view; the one, that of the oxide of lead as a *mordant*, (if we may use a term borrowed from the art of dyeing,) the other, that of the organic matter as a colorific agent. The former action, as more immediately interesting, claimed the first attention, and the more so, on account of the analogy which it seemed to offer with a similar *mordant* action of the chloride of silver itself, in Mr. TALBOT's curious process of successive alternate washes with salt and nitrate of silver, which by this time had been divulged by him. Of this action, the observed effect seemed to promise an explanation, should it be found to occur when other metallic oxides were substituted for lead, or other salts of this metal for the hydrate.

30. Paper washed with acetate of lead was therefore impregnated with various insoluble salts of that metal, such as the sulphate, phosphate, muriate, hydriodate, borate, oxalate, and others, by washing with their appropriate neutral salts, and when dry, applying the nitrate of silver, as usual. The results, however, were in no way striking as respects sensitiveness, in any case but in that of the muriatic applications. In all cases where such applications were used, a paper was produced infinitely more sensitive than any I had at that time made. And I may here observe that, in this respect, the muriate of strontia appeared to have decided advantage. I should observe that up to this time I had purposely abstained from repeating Mr. TALBOT's experiment above alluded to, being desirous of seeing what progress I could make in the inquiry without external aid. Comparative trials now made with some specimens of his preparing, satisfied me that in point of sensitiveness the two papers (with *mordant bases* of lead and silver) were nearly on a par*. In consequence of this I continued for some

* This does not refer to the later improvements made by Mr. TALBOT in the preparation of his paper, whether by the use of the bromurets or other means yet undescribed by him. I have already alluded to the exquisite sensitiveness of some specimens he has been obliging enough to send to me. It may be right to mention the date of the experiments described in (29.) and (30.) of the text, which are respectively April 19 and 27, 1839.

time to use (and at the time much to my satisfaction) my own process* in the preparations of paper for the copying of designs, as well as for the camera. By degrees, however, an inconvenience, little expected, appeared. The paper with a basis of lead turns yellow by keeping in the dark, and the tint goes on gradually deepening to a dark brown. But, what is very singular, this change is not equally rapid in all kinds of paper, a difference depending no doubt on the size employed, which, it may be observed here once for all, is of the utmost influence in all photographic processes. In one sort of paper (known by the name of *blue wove post*) it is instantaneous, taking place the moment the nitrate (if abundant) is applied. And yet I find this very paper to resist discoloration, by keeping, better than any other, when the mordant base is silver in place of lead. On the other hand, a paper of that kind called *smooth demy*, rendered sensitive by the process described in (28.), was found to acquire, by long keeping, a grey or slate colour, which increases to such a degree as might be supposed to render it useless. Yet in this state, when it is impressed with a photographic image, the process of fixing with hyposulphite of soda destroys this colour completely, leaving the ground as white as when fresh prepared. This fortunate restoration, however, does not take place when the paper has been *browned* as above described. Some of the muriatic salts also are more apt to induce this discoloration than others, especially those with earthy bases. But the effects in this respect are so capricious, that it is vain to attempt giving any connected account of them.

31. In consequence of this spontaneous discoloration†, I disused, for ordinary purposes, this mode of preparation, and adopted the following series of washes, on Mr. Talbot's principle, viz. 1st, nitrate of silver, S. G. = 1·096 (say 1·1); 2ndly, muriate of soda, 1 salt + 19 water; 3rdly, nitrate of silver, S. G. 1·132 (say 1·15), saturating the muriatic solution with chloride of silver, and occasionally dividing the last application into two consecutive washes of equal strength by dilution. This, as an ordinary working paper, is easily prepared, and has sensibility enough for most purposes. It gives very good camera pictures, and, if that particular sort of paper above named be used, it retains its whiteness well in the dark, at least for some weeks.

32. Paper of this kind may not always be at hand, nor under the same name may the same quality be always depended on, and every other sort I have tried discolours under this preparation, though much more slowly than when lead is used. Being much annoyed with this, I adopted for camera pictures a process which proved both convenient and effectual, and which applies equally well to both descriptions of

* 1st, or mordant wash, saturated solution of acetate of lead.

2nd, weak solution of common salt, containing $\frac{1}{20}$ of salt.

3rd, nitrate of silver, S. G. = 1·20, which however is best applied in two consecutive washes each of S. G. 1·10.

† Some camera pictures which I carried with me to Paris, and which were prepared on the 28th of April, were found on arrival much discoloured; and others prepared at the same time, were turned brown on my return from France about a month afterwards. (Neither had been fixed.)

paper. It consists in simply delaying the last or efficient wash of nitrate of silver, on which the sensitive quality depends, till the moment of using it, and in fact using the paper actually wet with the nitrate, and applied with its sensitive face against a glass plate whose hinder surface is in the focus of the camera. This affords other collateral advantages: 1st, that all crumpling or undulation of the paper is avoided; 2nd, that, being rendered in some degree transparent, the light is enabled to act deeper within its substance. If lead be used as the mordant basis of the paper, it is manifest that it can sustain no injury whatever by keeping or exposure, there being no ingredient on which the light can act, present, until the moment it is used*.

33. My attempts to generalize the property of lead above described, of acting as a mordant, have proved unsuccessful. Those metals only could be practically useful which give white combinations (*leucolytes*). Of these I found that paper impregnated with hydrate of bismuth, by wetting it with the nitrate of that metal and then soaking in water (by which it acquires a beautiful whiteness) comports itself as ordinary paper. The insoluble salts of mercury, even calomel, are also without effect, as indeed are many of those of silver itself, as the sulphate, borate, &c. I mean without effect in giving a high degree of sensibility; for in other respects they are far from inefficacious, as will presently appear.

34. It frequently happens that however carefully the successive washes are applied, so as apparently to drench completely every part of the paper, irregular patches in the resulting sheet will be of a comparatively much lower degree of sensibility, which degree is nevertheless uniform over their whole area. These patches are always sharply defined and terminated by *rounded* outlines, indicating as their proximate cause, the spreading of the wash last applied within the pores of the paper. They have been noticed and well described by Mr. Talbot, and ascribed by him, I think justly, to the assumption of definite and different chemical states of the silver within and without their area, which it would be highly interesting to follow out. They are very troublesome in practice, but may be materially diminished in frequency, if not avoided altogether, by saturating the saline washes used, previous to their application, with chloride of silver (see § 16.). By attending to this precaution, and by dividing the last wash of the nitrate into two of half the strength applied one after the other drying the paper between them, as recommended in Art. 30, Note, their occurrence may be almost entirely obviated.

35. With a view to ascertain how far organic matter is indispensable to the rapid discoloration of argentine compounds, a process was tried which it may not be amiss to relate, as it issued in a new and very pretty variety of the photographic art. A solution of salt of extreme dilution was mixed with nitrate of silver, so dilute as to form a liquid only slightly milky. This was poured into a somewhat deep vessel, at the bottom of which lay horizontally a very clean glass plate. After many days, the

* Mr. Talbot, to whom I communicated this process, wrote me word in reply, that he also had been led by his own experience to adopt a manipulation of the same kind though somewhat different in detail.

greater part of the liquid was decanted off with a siphon tube, and the last portions very slowly and cautiously drained away, drop by drop, by a siphon composed of a few fibres of hemp, laid parallel and moistened without twisting. The glass was not moved till quite dry, and was found coated with a pretty uniform film of chloride of silver, of delicate tenuity and chemical purity, which adhered with considerable force, and was very little sensible to light. On dropping on it a solution of nitrate of silver, however, and spreading it over by inclining the plate to and fro, (which it bore without disturbing the film of chloride,) it became highly sensitive, although no organic matter could have been introduced with the nitrate, which was quite pure, nor could any indeed have been present, unless it be supposed to have emanated from the hempen filaments which were barely in contact with the edge of the glass, and which were constantly *abstracting* matter from its surface in place of introducing new.

36. Exposed in this state to the focus of a camera, with the glass towards the incident light, it became impressed with a remarkably well-defined negative picture, which was direct or reversed according as looked at from the front or the back. On pouring over this, cautiously by means of a pipette, a solution of hyposulphite of soda, the picture disappeared, but this was only while wet, for on washing in pure water and drying, it was restored, and assumed much the air of a Daguerrotype when laid on a black ground, and still more so when smoked at the back, the silvered portions reflecting most light, so that its character had, in fact, changed from negative to positive. From such a picture (of course before smoking) I have found it practicable to take photographic copies; and although I did not, in fact, succeed in attempting to thicken the film of silver, by connecting it, under a weak solution of that metal, with the reducing pole of a voltaic pile, the attempt afforded distinct indications of its practicability with patience and perseverance, as here and there, over some small portions of the surface, the lights had assumed a full metallic brilliancy under this process. I would only mention further to those who may think this experiment worth repeating, that all my attempts to secure a good result by *drying* the nitrate on the film of chloride have failed, the crystallization of the salt disturbing the uniformity of the coating. To obtain delicate pictures, the plate must be exposed wet, and when withdrawn must immediately be plunged into water. The nitrate being thus abstracted, the plate may then be dried, in which state it is half-fixed, and is then ready for the hyposulphite. Such details of manipulation may appear minute, but they cannot be dispensed with in practice, and cost a great deal of time and trouble to discover.

37. This mode of coating glass with films of precipitated argentine or other compounds, affords, it may be observed, the *only* effectual means of studying their habitudes on exposure to light, free from the powerful and ever-varying influence of the *size* in paper, and other materials used in its manufacture, and estimating their degree of sensibility and other particulars of their deportment under the influence of reagents. I find, for example, that glass so coated with the iodide of silver is much

more sensitive than if similarly covered with the chloride, and that if both be washed with one and the same solution of nitrate, there is no comparison in respect of this valuable quality, the iodide being far superior, and of course to be adopted in preference for the use of the camera. It is however more difficult to fix, the action of the hyposulphites on this compound of silver being comparatively slow and feeble.

38. When the glass is coated with bromide of silver, the action, *per se*, is very slow, and the discoloration ultimately produced far short of blackness; but when moistened with nitrate of silver, S. G. 1·1, it is still more rapid than in the case of the iodide, turning quite black in the course of a very few seconds' exposure to sunshine. Plates of glass thus coated may be easily preserved for the use of the camera, and have the advantage of being ready at a moment's notice, requiring nothing but a wash over with the nitrate, which may be delayed till the image is actually thrown on the plate and adjusted to the correct focus with all deliberation. The sensitive wash being then applied with a soft flat camel-hair brush, the box may be closed and the picture impressed, after which it requires only to be thrown into water and dried in the dark to be rendered comparatively insensible, and may be finally fixed with hyposulphite of soda, which must be applied hot, its solvent power on the bromide being even less than on the iodide.

39. Analogous to the chloride, iodide, and bromide of silver, is the fluoride. I have not experimented on it, but should it be decomposed by light, either *per se*, or aided by the nitrate, it seems extremely probable that the nascent fluorine or hydrofluoric acid disengaged, finding itself in contact with glass, might corrode it, and thus produce *an etching*, a result well worth some trouble to obtain.

40. Of organic salts of silver precipitated on glass, I have tried several. The results are very widely different from those obtained by washing over papers, and will form a separate subject of study. Some of them (as the oxalate) form very even and most delicate coatings, highly sensible to light, and, what is remarkable, differing greatly in this respect according as the side next the glass, or that in contact with air, is exposed to it.

41. Light has long been known to reduce the salts of gold as well as silver, and I have shown* that platina in some of its combinations is also very powerfully affected by the same agent. From the use of these metals it was reasonable to look for results of an interesting nature. It might, for instance, be expected that by applying the chloride of one or other of them as a ground or mordant for the reception of a wash of an argentine solution, the reduction of both the silver and the mordant metal would take place, and thus a more rapid and intense blackening would be produced than in the case of silver alone. The well-known instability of the oxide of gold under deoxidizing influences generally, and the intense colouring power of this metal would, *à priori*, render it probable that cases of unstable equilibrium might occur, which the action of a very feeble light might overset.

* See London and Edinburgh Philosophical Magazine, New Series, No. 1, 1832.

42. Papers were washed with the chlorides of gold and of platina, freed from excess of acid. In the case of platina, they proved insensible; in that of gold, a slow but regularly increasing darkening takes place, and the paper at length, under the influence of light, becomes purple. A wash of nitrate of silver increases, but not to a high degree, the sensibility of the aurated paper, but that impregnated with platina remains insensible, unless the nitrate be in great abundance.

43. In the chlorides both of gold and platina, nitrate of silver produces precipitates. In that of gold, the precipitate is of a yellow-brown colour, and in that of platina, a pale yellow. Both are possibly metallic double salts in which the gold or platina, as well as the silver, is in the state of chloride. As relates to the action of light, I find the precipitate of gold very little sensitive if spread on glass, that of platina not at all. On paper, the aurated precipitate is blackened somewhat more readily.

44. If paper impregnated with oxalate of ammonia be washed with chloride of gold it becomes, if certain proportions be hit, pretty sensitive to light; passing rather rapidly to a violet purple in the sun. It is next to impossible to dry paper so prepared, however, as a very gentle heat blackens it. It passes also to the same purple hue in the dark, though much more slowly; so that, as a photographic combination, it is useless.

45. Paper impregnated with acetate of lead, when washed with perfectly neutral chloride of gold, acquires a brownish yellow hue and a sensibility to light which, though not great, is attended with some peculiarities highly worthy of notice. The first impression of the light seems rather to whiten than to darken the paper, by discharging the original colour, and substituting for it a pale greyish tint, which by slow degrees increases to a dark slate colour. But if arrested while yet not more than a moderate ash-grey, and held in a current of *steam*, the colour of the part acted on by light (and of that only) darkens immediately to a deep purple. The same effect is produced by immersing it in boiling distilled water. If plunged in cold water the same change comes on more slowly, and is not complete till the paper is dried by heat. A *dry heat* however does not operate this singular change. The best way of making this and similar experiments is to shade one half the paper operated on with an opake screen closely applied, and to reserve portions, before and after exposure, for comparison.

46. This is not the only case in which the impression made by light is rendered more evident by subsequent applications. A solution of chloride of platina in ether being washed over a bibulous paper impregnated with hydriodate of potash, in certain degrees of strength and copiousness, browns pretty rapidly in the dark, but much more rapidly and to a much deeper tint in sunshine. A paper so washed and partly shaded, on exposure produced a well-defined figure of the shading body, which, on the addition of a fresh wash of the hydriodate, *out of the light*, became much more strongly contrasted with the surrounding ground.

47. Paper (No. 644) washed with acetate of lead and then with chloride of platina, is absolutely insensible, and only becomes very feebly sensible when thoroughly

impregnated with nitrate of silver. But if, in place of the nitrate of silver, a wash of hydriodate of potash be superadded, the effect is remarkable. If the hydriodic solution be strong and plentiful, the paper is immediately coloured dark brown, whether in light or darkness. If very weak, no effect; but if applied of a certain intermediate strength, though not *immediately* affected in the shade, yet, if held (while wet) in the sun, it darkens with extraordinary rapidity to the same deep brown hue, *and presently after, the exposure to the sun continuing, whitens again.* A fresh dose of the hydriodate being applied, it again darkens, but is no longer capable of restoration, and the darkness then goes on increasing to a fine deep chocolate brown. The paper with which I have found this experiment to succeed best is called "smooth wove demy." It requires a clear and steady sun. If gold be substituted for platina, the hydriodic salt exercises, in virtue of its ordinary chemical relations, so powerful a darkening effect, that the influence of light can hardly be traced; but, as far as can be ascertained, something of the same sort obtains here also.

48. In the experiment of the last article we see that the influence of the hydriodic salt, contrary to its usual habitudes (see Art. 17), is to exalt the deoxidizing action of the light, and even to call that action into evidence where it either did not exist, or was masked before. This is not, however, by any means a singular instance. Hydriodate of potash, when very weak, acts in combination with nitrate of silver to produce a paper much more sensitive than the nitrate alone, the remarkable peculiarities of which will be noticed further on. A paper endowed with a pretty high degree of sensibility may also be prepared with the following triple application, viz. 1st, Acetate of lead; 2nd, Hydriodate of potash; 3rd, Nitrate of silver. And here also a phenomenon, something of the same kind as was described in the subsequent part of the experiment of the last article, occurs; for if paper so prepared and darkened in the sun be washed over with a fresh dose of the hydriodate, the exposure to sunshine being continued, it whitens with great rapidity*; and were it practicable (which I have not found it) to ensure precisely the same ingredient-proportions and the same degree of blackening in the sun to start from, I should not hesitate to propose this as an excellent process for a *positive* photographic paper*. A still more remarkable example of the power of this salt to stimulate the *darkening* action of the sun's rays, is to be found in its combination with the tartrate of silver. But of this more hereafter, when we have become better acquainted with the separate action of the different elementary rays, which it is necessary to study before we can understand the complex relations which enter into these phenomena. To this therefore I hasten.

IV. *Chemical Analysis of the Solar Spectrum.*

49. That rays of different colours and refrangibilities have not all an equally energetic action in effecting chemical changes, has long been understood; and that the

* Another paper which possesses this property in an eminent degree is prepared with 1st, acetate of lead; 2nd, sulphate of soda; 3rd, nitrate of silver, darkened in the sun and washed with the hydriodate.

most energetic in the reduction of argentine compounds are those which have been hitherto regarded as extra-spectral, or as lying beyond all visible illumination on the more refrangible side, is a proposition familiar to photologists. How far this opinion is strictly correct I shall take occasion presently to examine. It has also, on the ground of Dr. WOLLASTON's experiments on guaiacum, been received, that the less refrangible end of the spectrum produces chemical changes of a certain kind; but as that eminent philosopher found changes apparently of the same kind, in that substance, to be operated by a gentle heat, unaccompanied with light, and as those changes were, in consequence, referred by him to the calorific rays, which we know to accompany that end of the spectrum, rather than to any peculiar chemical action, this point must be considered as hitherto undecided. The facts about to be stated, however, leave no further doubt on the subject.

50. The experiments described in my first communication on this subject, in which coloured and other media were used to analyse the incident light by absorption, are quite sufficient to show a high probability at least that the chemical energy is distributed throughout the spectrum in such a way as to be, by no means, a mere function of the refrangibility, but to stand in relation to other physical qualities, both of the ray and of the analysing medium, and *that* relation by no means the same as that which determines the absorptive action of the latter on the colorific rays. The experiments I am about to describe will show that there is also a third set of relations concerned in this action, and most materially influencing both the amount and character of the chemical action at each point of the spectrum, viz. those depending on the physical qualities of the substance on which the rays are received, and whose changes indicate and measure their action. This indeed might be expected, or rather it would seem unlikely, on a general view, that the contrary should be true, seeing that some substances are, others are not, affected by light. But no such general and *à priori* reasonings would, I think, lead us to many of the conclusions actually arrived at; as, for instance, that one and the same spectrum thrown on papers differently prepared, shall indicate by the impressions photographically left on those papers, the most capricious differences in the *scale of action*, whether estimated by the extent of the discoloration in the direction of the length of the spectrum, or by its intensity at different points of that length. We could hardly have predicted, *à priori*, for example, that, acting on one description of paper, the chemical spectrum (as indicated merely by the length of its photographic impression,) should include within its limits the *whole* luminous spectrum, extending much beyond the extremest visible red rays on the one hand, and on the other to a surprising distance beyond the violet; while, if another paper be used, all action should appear definitively cut off at the orange; if another, at the commencement of the green; and if another, at that of the blue rays: *that*, in the case of one description of paper, the maximum of apparent action should lie *beyond*, in that of another *within* the visible violet; in a third far in upon the blue; while in a fourth several successive maxima should appear. Still less would any

à priori conclusion prepare us for differences in the *kind* of action at different points of the spectrum, as manifested by differences of colour produced, or for what at first sight appears yet more singular, the *shifting upon the spectrum itself* of the points where these differences of action commence and terminate. Nor, lastly, could any penetration enable us to anticipate a fact so totally at variance with all our ideas, as that two rays of different refrangibilities, (and therefore of different lengths of undulation) acting at once, should produce an effect which neither of them acting separately is competent to produce at all; a fact which seems difficult to deal with on any theory of light. These are but a few of the many singular results which promise to render this field of research, hardly yet entered upon, as replete with novelty, variety, and interest, as any other department in the vast field of physical optics.

51. My first experiments on this subject were directed to the detection of inactive spaces (if any) in the chemical spectrum, analogous to the dark lines of Wollaston and Fraunhofer in the luminous one. Possessing at the time no means of fixing a sunbeam of large area, I operated coarsely with a prism placed horizontally. The light was introduced through a narrow slit, and the time chosen for experiment when the sun was on the meridian, so that no motion of the spectrum in the direction of its length might confound the result. This arrangement proved, as indeed I expected, quite inadequate to the end proposed; but it sufficed to reveal several leading facts, viz. 1st, That with the paper employed (which happened to be a specimen prepared by the process in art. 31. on Mr. Talbot's principle) the maximum of action was not beyond the violet, as I had always understood it to be, but rather about the confines of the blue and green, nearly where Fraunhofer's ray F is situated. 2ndly, That the extent of the chemical spectrum beyond the visible violet light far surpassed any idea I had previously formed of it. For, in fact, the visible termination of the violet rays nearly bisected the photographic image impressed on the paper. 3rdly, That *in* the visible violet rays there occurred a sort of minimum of action, about one third of the way from Fraunhofer's ray H towards G. 4thly, That the whole of the red, up to about Fraunhofer's line C, appeared to be inactive. And 5thly, That where the orange-red rays, or the less refrangible half of the portion from C to D in Fraunhofer's spectrum, fell, there was communicated to the paper a sort of ruddy tint, a dull brick red.

52. From this experiment, coarsely performed as it was, several practical conclusions came to be deduced. 1st, The absolute necessity of an achromaticity in the object-lens of a photographic camera, of a very perfect kind, and in some sort *sui generis**, is rendered evident by the great extension of the chemical spectrum. And it is equally clear that this very extension affords the most perfect and certain means

* A lens photographically perfect, or which unites all the chemical rays into one focus, may be called *amacratic* (*αμα together*, *κρατος power*) or *amasthenic* (*σθενος force*). If this nomenclature be adopted, a *diacratic* or diasthenic medium will be one which transmits the chemical power or force; *diacratescence* that quality in virtue of which it does so, &c. &c. In place of the awkward word *diathemanëity*, however, I would venture to propose its Latin version, *transcalescence*.

of testing the perfection actually attained in this respect. Neither the refractive nor dispersive indices best adapted for an ordinary double achromatic lens are exactly those most fitted to collect the chemical spectrum. To determine those which are so, the method pointed out in my Treatise on Light* seems, on the whole, best adapted, viz. by over correcting the crown lens, by a flint one somewhat too concave, and then separating the lenses, till it is found on trial that the adjustment is complete. The dispersive ratio best adapted to the purpose will then be found by the formula there stated. For the refractive indices, those corresponding to FRAUNHOFER's ray F will probably suffice.

53. Another and highly important practical conclusion which seems to be pointed to by this experiment, is the possible future production of naturally coloured photographic images. The feeble traces of colorific power above noticed might become exalted by the use of other combinations, which it therefore became extremely interesting to vary in every possible way. Mr. TALBOT has already observed, that in copying, photographically, designs on coloured glass, the *red* portions would *occasionally* impart a corresponding tint to the impression of that particular portion of the design so coloured. The *rationale* of this fact, as will afterwards appear, is somewhat more complex than it would seem to be on perusing the experiment just cited. The bright red varieties of coloured glass by no means transmit the red rays in a state of purity; and those varieties which do so or nearly so, are incapable of producing the effect in question, if the paper be fresh prepared and completely free from all traces of previous photographic action. At the moment, however, the direct explanation seemed satisfactory, and proved an incentive to further inquiry.

54. July 9, 1839.—A highly concentrated spectrum was therefore formed, by receiving the rays after emergence from the prism used in the last-described experiment

1. Colours of the luminous spectrum.	2. Colours impressed on the paper.
Extreme red†	None.
Mean red	None.
Orange	Faint brick-red.
Orange-yellow	Brick-red, pretty strong.
Yellow	Undecided; red passing into green.
Yellow-green	Dull bottle-green.
Green	Dull bottle-green, passing into blueish.
Blue-green	Very sombre blue, almost black.
Blue	Black, but when the exposure was long continued, metallic yellow, like an imperfect gilding.
Violet	Black, passing into the same metallic yellow by long exposure in the less refrangible portions of the violet ray.
Beyond the violet	Violet-black, or purplish-black.

* Encyclopædia Metropolitana, art. LIGHT.

† As in this table I use for the first time in this paper the expression "extreme red," it will be well to define what is meant here and in the sequel by that term. The extreme red rays intended, then, are those which are brought into view by defending the eye while looking at the spectrum with one or more thicknesses of that sort of deep blue glass which is common enough in commerce, and which I am told owes its colour to smalt.

(a beautiful crown-glass prism by FRAUNHOFER), on a large crown lens, and receiving the focus on paper prepared as in Art. 31. The result was equally striking and unexpected. A very intense photographic impression of the spectrum was rapidly formed, which, when withdrawn and viewed in moderate daylight, was found to be coloured with sombre, but unequivocal tints, imitating those of the spectrum itself. These tints were as stated in column 2 of the preceding Table, the colours at the corresponding points of the luminous spectrum being those in column 1.

I have not succeeded in fixing these tints. They are, however, susceptible of half-fixing by the mere action of water, and may be viewed at leisure in moderate daylight, or by candlelight. And what is not a little remarkable, instead of fading, by keeping in the dark, they become much more evident and decided after a few days. And this, I would observe once for all, is a phenomenon of constant occurrence, whatever be the preparation of the paper used, i. e. when colours are produced at all.

55. The metallic yellow, induced by the blue and least refrangible violet rays, seems to mark the extreme action of the solar light on this particular preparation of paper, and perhaps on one or two other sorts, by the formation of a pellicle of metallic silver thick enough to reflect light with some degree of copiousness. Accordingly, this pellicle is apt to appear as a *bright* border or framing round the photographic copies of engravings, where not covered by the original, and when long exposed, and, in some cases, even in the strong lights of the pictures themselves.

Extension of the visible Prismatic Spectrum.—A new Prismatic Colour.

56. A variation in the experiment of Art. 54. deserves to be described, not so much on account of its bringing into view any photographic novelty, as for its confirming and placing in distinct evidence a fact which I have long more than suspected, but have always hitherto hesitated to announce. The fact to which I allude is that of the existence of *luminous* rays beyond the violet, but not affecting the eye with a sensation of violet, nor of any other of the recognised prismatic hues, but rather with that colour which is commonly termed lavender-grey; at least such is the impression

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i. e. to cobalt. This glass insulates two red rays of definite refrangibilities; one which I call the mean red, the other the extreme red. This latter is situated so decidedly at the extremity of the visible spectrum, that if a dot be made in the centre of the well-defined and round solar image to which it corresponds, and the glass be then laid aside, that dot is judged by the eye to lie exactly at the end, or if anything rather beyond than within the end of the visible spectrum. This is the ray which forms the subject of a communication to the Royal Society by Mr. COOPER (read May 16, 1839), but which I have pointed out to the especial notice of photologists on several occasions, viz. 1st, in my paper on the action of crystallized media on homogeneous light, read December 23, 1819, and printed in the Philosophical Transactions for 1820 (See vol. cx. p. 88); 2ndly, in a paper read November 18, 1822, before the Royal Society of Edinburgh, and printed in the Transactions of that Society for 1822; 3rdly, in my Treatise on Light (Encyclop. Metropol. §§ 487, 497, 503.). This ray has, in fact, been constantly used and recommended by me as a standard ray, for which purpose it is admirably adapted on account of the facility of insulating it in a state of purity.

they produce on my own eye, as well as on that of a friend to whom I have shown them, and who proposed that term, or that of simple grey, to express it. It was in the course of my experiments on the polarized rings in 1819, made with M. Biot's apparatus, that I was first led to something approaching to conviction of the existence of such rays. While following out the measurements of the angular diameters of the polarized rings in those experiments, it was found easy to arrive at a maximum diameter of each ring, corresponding to an extreme red illumination, but by no means so for the other end of the spectrum. Long after any sensible violet illumination continued to fall on the reflector of the apparatus, that spark of light, increasing, diminishing, vanishing, reappearing and vanishing again, as the crystal was turned round, on which the observation in question depends, would still remain obstinately visible, and the angular diameter of the polarized ring deduced from it would still go on decreasing till it became evident that there was no assigning any definite limit to the violet end of the spectrum, otherwise than by the use of a definite absorptive medium,—I mean a limit where the visible phenomena depending on continually decreasing breadths of undulation should be held to cease. It was equally evident in those experiments, that the colour of this outstanding ultra-violet ray was not properly that of any received prismatic tint; but this, at the time, I attributed to the great and progressively increasing faintness of the rays in question not making sufficient impression on the eye to allow of any decision as to their colour. This view of the matter, however, is incorrect. The following experiment will show, that these rays when so concentrated as to possess an unequivocal illuminating power, still show no *colour*, but that sort of imperfect white which is best distinguished by the terms grey, ash-colour, lavender-colour, or such expressions. As orange, indigo, and violet, vegetable tints, are used for those of the prismatic hues, I may be allowed to express by the epithet lavender the rays which produce the tint in question, rather for the purpose of abbreviating the uncouth appellation of *ultra-violet*, and avoiding the ambiguity attaching to the term *chemical* rays (which exist in all regions of the spectrum) than for that of laying any undue stress on the observed fact.

57. A spectrum from a highly refractive and dispersive flint-glass prism, the companion to the crown prism before mentioned, and, like it, the personal gift of Fraunhofer, of faultless purity and perfect workmanship, was thrown on the same crown lens as in the last experiment. This was purposely placed at once oblique to the incident light, and out of the plane of dispersion of the prism, or that plane to which its refracting edge is perpendicular. Moreover, it was so arranged that the spectrum might fall on any part of the lens, above, below, or on either side of its centre. Under these circumstances, it is easy to give excessive concentration to light of any given degree of refrangibility, the rest being thrown aside into long, coloured caustics. In fact, the image of the spectrum is thus bent and distorted into curvilinear forms, the general type of which may be assimilated to the letter C lengthened and flattened, with ends more or less unequal. And upon these curves, so opened or contracted,

the colours may be made to occupy any spaces, by receiving the spectrum higher or lower on the lens, and by inclining its axis more or less to the plane of incidence. Without calling coloured figures to aid our description, we may conceive the matter perhaps most readily by considering the spectrum as painted on a ring, or on some oval curvilinear form, having the eye more or less elevated above its plane, and capable of being made to revolve about its centre, the spectrum, including the chemical rays, occupying about two thirds of its circumference. If we conceive this ring, oval, or curve, projected on any plane, it is clear that the colours as depicted on that plane will have more or less concentration, according as the element of the arc corresponding to each is more or less inclined to the plane, and that where that inclination is perpendicular, the intensity at the point of projection will be (mathematically speaking) infinite in comparison with that of any other portion of the spectrum.

58. If we conceive the eye to be situated in the plane of our fictitious ring, its projection will be rectilinear, and we shall have a spectrum, as it were, doubled back on itself at any particular part of its length, and the density of the rays where this reduplication takes place will be vastly greater than at any other point. By placing the lens, therefore, so that this condition shall hold good, (which happens when its axis lies in the plane of refraction of the prism, and the spectrum falls on its principal section,) the photographic impression will exhibit in its highest intensity the effect of that particular chemical ray which is thus violently concentrated. Its illuminating power, if any, will thus also be brought into corresponding evidence.

59. By this arrangement, the colours described in Art. 54. were brought out in great distinctness, and some very striking appearances produced. Now when the ray whose chemical and optical qualities were thus especially forced into prominence was among those whose place in the spectrum, though far beyond the ordinarily visible violet, was yet within the limit of the chemical action, the portion of it thus concentrated became very apparent to the eye as a greyish-white insulated oval spot, which, when first noticed, was for the moment attributed to some accidental achromatic reflection of light backwards and forwards within the prism, an idea which the smallest movement of the prism or the lens sufficed to dissipate. But if such an idea could have been entertained after a moment's thought, the reception of the spectrum on a slip of prepared paper completely refuted it, by the intense chemical action exercised at that particular spot. The blackening, indeed, was there instantaneous, as if a red-hot body had been applied behind, or a smoky flame directed on the paper over all the space so visibly illuminated, and faithfully marking its form and degradation. A few moments of exposure sufficed to connect this spot (at first insulated) with the general impression of the spectrum, which by rapid degrees came into view. We come now to other and more remarkable results.

Chemical Properties of the Red end of the Spectrum.

60. August 27, 1839.—A highly concentrated spectrum having been formed on muriated paper, and kept for some time on the same spot, a coloured picture was formed, as in Art. 54. On examining this picture by dispersed daylight, I now for the first time noticed, that in all that part of the paper on which the full red of the spectrum had fallen, there was an appearance of whiteness, a sort of white prolongation of, or appendage to, the dark photographic impression, contrasting itself with the general ground of the paper, which, under the influence of dispersed light from passing clouds, sky, &c., had acquired a very sensible discoloration. This discoloration had, therefore, as would appear, been counteracted by the red rays of the spectrum, which are, therefore, by no means to be regarded as inactive, but rather, at least in this instance, as exciting an action of an opposite nature to that of the blue, violet, and lavender rays. To generalize this conclusion, and to assume a kind of positive and negative polarity in the spectrum, was the first and natural idea which presented itself on making the above experiment. Such a polarity had been suspected—asserted indeed—either on *à priori* grounds, or on the strength of vague and loose reasonings founded on the general aspect of many natural phenomena. But almost the very first step in the further progress of the inquiry sufficed to prove the existence of something beyond a mere opposition of qualities.

61. When paper already somewhat discoloured, by a short exposure to direct sunshine, was used to receive the spectrum, this whiteness was no longer found to be produced; but instead of such an appearance, the red tint, already noted as fringing the least refracted end of the photographic spectrum, was found to be not only much more lively, but to extend further; and instead of terminating at or near the limit where the red and yellow blend into orange, as in the Table in article 54, to encroach deeply on the space occupied by the red rays, and even, in some instances, entirely to cover that space. When, instead of exposing the paper in the first instance to direct sunshine, it was rendered considerably dark by exposing it in the violet rays of a prismatic spectrum, or in a sunbeam which had undergone the absorptive action of a solution of ammonio-sulphate of copper, the phenomenon in question was developed in great perfection; the red rays of the condensed spectrum producing on such paper, not whiteness, but a full and fiery red, which occupied the whole space on which any visible red rays had fallen.

62. In further illustration of this peculiar action, I therefore made the following experiment. On a piece of paper strongly darkened by exposure to the sun under a green glass which was ascertained to absorb every trace of the extreme red, I fastened a standard combination of glasses which transmitted *only* that red, being the identical combination described in my paper* above cited, and the same which I have always used in my experiments to insulate that ray in its utmost purity. The

* Philosophical Transactions, 1820.

paper so covered was exposed for a day to desultory and feeble gleams of sunshine, and when withdrawn and compared with a portion of the same piece of paper preserved in the dark, to have become materially less dark than before, and to have acquired a high red tint instead of the greenish blackness it had before. Papers darkened by exposure under cupro-sulphate of ammonia and by free exposure, were reddened in exactly the same way.

63. It was natural, perhaps, in this stage of the inquiry to regard, as I did, this change of tint, accompanied as it was with a diminution of total intensity, as a first step in the process of ultimate restoration to perfect whiteness, and that I should, in consequence, in the way of practical application, have forthwith proceeded to attempt the production of a *positive* photographic copy of an engraving by placing it on a frame, as usual, over a paper blackened by exposure to sunshine, covering the whole with a dark red-brown glass previously ascertained to absorb every ray beyond the orange. In this way a copy *was* obtained in effect, so far of a positive nature that the shades of the original engraving were expressed in black. But the lights, instead of *white*, were found to be eaten out in red, the colour of venous blood. Beyond this I could not succeed in pushing it, nor could I obtain any other result on exposing blackened paper under the standard red glass, however long continued.

Combined action of rays of different degrees of refrangibility.

64. It is evident from this experiment, that the conservative whitening, or oxidating action of the red rays depends on some other principle than a mere contrariety of action to that of any one species of ray in particular. It is proved, 1st, that a very powerful red ray is capable of neutralizing, *in the moment of its action*, the photographic effect of a feeble white one, in which, besides the red, are contained in due proportions all the other rays. But on the other hand, it is equally shown, that a long-continued *subsequent* action of the red rays is incapable of destroying entirely the effect of other rays once fully produced, though it superinduces on that effect the very remarkable modification of assimilating the blackness arising from their action to its own proper colour. A question of high interest therefore arose, as to what would be the effect of the combined action of a red with any other single ray in the spectrum, *if acting at the same instant*; or more generally, whether any, and what differences subsist between the joint and the successive actions of any two given rays of different and definite refrangibilities, and whether the joint action be capable, or not, of producing effects which neither of them acting alone would be competent to produce.

65. As regards the *successive* action of differently coloured rays, I have not been able to perceive, hitherto, any influence which the prior action of the less refrangible rays of the spectrum exercises in modifying the subsequent effects produced by the more refrangible. That portion of a slip of paper, for example, which has been preserved in a state of whiteness under the action of dispersed light by the long-continued action of red light, is darkened quite as rapidly, and assumes the same shade

of blackness under the subsequent influence of a blue, violet, or lavender ray, as it would have done had no previous exposure to the red rays taken place. As far as my experiments go, also, it appears that every part of the spectrum impressed by the more refrangible rays, is equally reddened, or nearly so, by the subsequent action of the less refrangible.

66. On the other hand, as regards the question, whether or not the simultaneous action of two rays of different refrangibilities be capable of producing photographic effects which neither acting separately is competent to produce, the following experiment will afford a satisfactory affirmative answer. A very fine and concentrated spectrum was formed, by receiving on an achromatic lens (by CAUCHE of Paris) of three inches aperture and twenty-six inches focal length, a sunbeam previously separated by the FRAUNHOFER flint prism above mentioned (Art. 57.). The screen on which this spectrum was received having been carefully adjusted to the correct focus, a thin flint prism of small refracting angle was interposed between the lens and its focus, so as to intercept all the less refrangible rays from the green to the extreme red, and to throw them upwards by a second refraction, thereby superposing the image of the less refracted portion of the spectrum on that of the more refracted formed by a single refraction. And by varying the place of the second prism in respect of its distance from the screen, this superposition might be made to take place at any given point of the more refracted spectrum. Things being thus arranged, a slip of muriated paper was fixed on the screen so as to receive the simultaneous action of the rays in question. The results were very striking and beautiful. The *blackening* power of the more refrangible rays seemed to be suspended over all that portion on which the less refrangible fell, and the shades of green and sombre blue which the latter would have impressed on a *white* paper, were produced on that portion which, but for their action, would have been merely blackened. But in place of the dull brick-red fringe* which would have bounded this part of the spectrum on a white ground, and in place of the white prolongation (Art. 60.) which would in that case have marked the region of the mean and extreme red, there was produced a full, rich and fiery red, occupying the whole of that region, and passing into a sombre crimson or purple, where the action of the extreme red rays ceased to counterbalance or modify that of the more refrangible ones, in concert with which they acted. The general effect was not unlike that of the succession of tints in the second series of the Newtonian rings, or rather, of the transition from the third into the second, only with a less vivid development of every colour but the red.

67. To those who may be disposed to repeat this and the following experiments, I would here observe that a polar axis driven by clockwork, to follow the diurnal movement of the sun and fix the spectrum, is indispensable. But, as the last niceties of astronomical observation have as yet no place in this research, the interrupted motion of a pendulum clock, which is fatal in such observations, is not here

* See Table in Art. 54.

felt as an inconvenience; so that by a very simple contrivance, founded on the principle of counterbalancing every part of the apparatus, and employing the clock only to regulate and controul the action of a driving weight, and *not to do the work* of putting the apparatus in motion, I was enabled with very little trouble to adapt such a movement to an axis of good workmanship which had formerly served as the support of a BIRD's quadrant. This being fixed in the proper position, carried round, diurnally, a frame on which were attached, with every necessary adjustment, the FRAUNHOFER prism, the achromatic lens and the focal screen above mentioned, as well as any other occasional and needful apparatus for varying the experiments, especially for interposing coloured glasses, and plate-glass rectangular boxes, destined for the reception of coloured and other liquids, whose absorptive action on the chemical rays it should be desired to try.

68. In my first experiments with an apparatus thus disposed, I employed a large prism of plate glass filled with water, for the sake of operating on a large beam of light. But however large the refracting angle, the dispersion of water proved too low, the spectrum being too short, in proportion to the sun's angular diameter, to give distinct and sharply defined results. It was therefore disused, and the FRAUNHOFER flint prism substituted: and here I may take occasion to mention, that whether water, crown, or flint glass was used, I was not sensible of any *peculiarity* of action (apart from difference of dispersive power) which I could fairly attribute to the qualities of those media. I confess I had looked for a contrary result, and had speculated, with somewhat excited expectation, on the discovery of bands, or interruptions, or points of maximum and minimum action in the spectrum, traceable to unequal absorption of the chemical rays by those media, though colourless. None such however appeared. But on this point I would hardly be understood to speak, as yet, decisively in the negative.

69. One of the first uses to which this apparatus was applied was an endeavour to ascertain the existence or nonexistence of fixed lines in the chemical spectrum analogous to those in the luminous, and whether corresponding, or not, in place. To this end it was necessary to diminish the sun's angular apparent diameter, which was done by affixing a small achromatic telescope with a low magnifying power, in a reversed position (eye-piece towards the sun), to receive and refract the rays before their incidence on the prism. By this means, spectra were obtained whose lengths were in a highly increased proportion to their breadths. In one experiment this proportion (as ascertained by measurement of the impression left) was so high as 67 : 1. But in this, as in other experiments where a less degree of dilatation was obtained, the interruptions, if any, to the continuity of chemical action from end to end, were so equivocal that I cannot venture to assert their existence positively, though I am led to believe it from the fact of some slight inequalities in the violet region having been repeated, in the same place of the spectrum, on shifting the paper to eliminate the possible influence of streaks left by the brush in its preparation. The only cer-

tain conclusion then that can be drawn from these trials is, the nonexistence of any *large* inactive spaces in the spectrum.

70. With the apparatus in its ordinary state, the following are the dimensions of the several principal elements of the luminous spectrum, which it will be necessary to bear in mind as data in the experiments to be detailed. They are given in parts of a scale each equal to $\frac{1}{10}$th of an inch, and are reckoned from a point, selected as a fiducial centre, at that particular definite yellow which the cobalt glass above noticed insulates in great abundance and with considerable sharpness of termination*. It was not mere caprice which determined me to depart in this particular instance from my hitherto invariable standard, the extreme red ray. The solar image in that ray is far better defined than in the yellow in question. But from many phenomena observed in the action of coloured glasses I was led to suspect, and the suspicion was borne out by the results, that this ray may be considered as marking a sort of chemical centre, a point of equilibrium, or rather of change of action, in the spectrum. In fact, in a large proportion of the preparations submitted to the action of the spectrum, that action, as far as it is marked on the paper by traces of darkness, terminates precisely, or very nearly indeed, at this point, allowing for the sun's diameter.

Dimensions, &c.	Parts each $\frac{1}{10}$ inch.
Focal length of the achromatic lens used	774
Breadth of the fixed spectrum, or diameter of the sun's image	7·20
Distance *from* the fiducial point or centre of the sun's image formed by the yellow ray, 1st, *to* the centre of the image formed by the extreme red†	− 13·30
2nd, *to* that of the image formed by the mean red	− 6·66
3rd, *to* that of the image formed by the last violet transmitted by the same glass, a tolerably well-defined ray; the centre of the terminating semicircle of the spectrum judged of as well as practicable	+ 40·62
Total length of the luminous spectrum from centre to centre of the terminating solar images	53·92

71. It would be mere waste of time to recount the almost numberless experiments on particular preparations of paper made with this apparatus, especially as they lead to no general laws. At the same time, a number of interesting facts have been encountered in particular cases; and we are led to form a very high idea of the vastness of this new field of inquiry, and the extensive bearings and connexions of any theory which shall give a rational account of these complex phenomena, from the way in which these facts, so various and so remarkable each in itself, stand insulated from one another. I shall now therefore proceed to state the phenomena observed on pre-

* See Transactions of the Royal Society of Edinburgh, 1822.

† The sign − indicates that the distance is measured towards the least refracted end of the spectrum, the sign + towards the most refracted.

senting some particular preparations to the action of the spectrum. And to begin with the simplest case, that of the nitrate of silver alone.

72. *Nitrate of Silver.* (Paper No. 570.*).—Smooth wove demy paper washed with a solution, specific gravity 1·132. The general colour of the spectrum is a rather pale brown inclining to pink, and the tint is nearly uniform, except towards the most intense part (which falls about the middle of the blue rays), where it somewhat inclines to a dark grey or lead colour, while, on the other hand, at the violet extremity of the spectrum it more inclines to pink. The total length of the spectrum impressed on white paper, as well as the insensible gradation of its most refracted end will admit of measure, is about eighty-five parts. Its least refracted termination (at which it shows no sign of red, green, or blue tints) is at − 3·2, which allowing 3·6 for the sun's semidiameter, may be considered as placing the limit of action almost exactly at the fiducial point. The maximum of intensity occurs at + 23 parts, or thereabouts; and there is a considerable indication of a second maximum at about + 43 parts, after which it degrades very rapidly, as we advance into that region of the spectrum which we have designated as the lavender rays. The feebleness of action of these rays, as compared with that of the blue and violet, is indeed the chief characteristic of this spectrum.

73. When the spectrum is made to act on paper similarly prepared, but previously discoloured by exposure to sunshine under cupro-sulphate of ammonia, the phenomena are materially different. The photographic spectrum is lengthened out on the red or negative side by a faint but very visible red portion, which extends to − 13·7, or fully up to the end of the red rays as seen by the naked eye. The tint of the general spectrum, too, instead of brown is dark grey, passing, however, at its most refracted (or positive) end into a ruddy brown. The maxima occur nearly at the same points as above stated, but that at the end of the violet is more unequivocal, and the degradation beyond it more sudden.

74. *Muriate of Soda*†. (Papers Nos. 545, 559, 568.)—The spectrum is very long, intense, and far more variously coloured than any other which has hitherto occurred in my experiments. If the paper be purely white and the day cloudless, the visible impression commences with a pretty high red at − 7·6, which at about half that distance (− 3·8) below the fiducial point begins to pass into green, through a kind of livid mixed tint. The best green, which is, however, of a sombre and dull character, is developed at or a little above the fiducial point, covering a breadth of about four parts. Thence, with a barely perceptible tinge of dark blue, it passes rapidly into intense blackness, which at + 80 begins to die away into a purplish brown, and

* These numbers refer to memoranda kept of the preparation of the various papers used.

† When one ingredient only is thus mentioned, it is understood that that ingredient has been applied as an intermediate wash between two washes of nitrate of silver; thus the preparation of the paper No. 559, on reference to the memoranda kept, appears to have been, 1st wash, nitrate of silver 1·132; 2nd ditto, muriate of soda, 1 salt + 19 water; 3rd ditto, nitrate of silver 1·096. If the word *lead* be added, it is to be understood that the first wash has been acetate of lead.

terminates the spectrum at + 90·23, making the total length of the darkened or discoloured impression 97·83 parts.

75. If there be dispersed light, the spectrum is lengthened out by a pinkish white addition extending below the fiducial point as far as − 22·43; so that the whole extent of the action on this paper is 112·66, being considerably more than double the total length of the ordinary luminous spectrum*.

76. It should be observed that, as respects the colours, the nature of the paper itself is far from indifferent. The thin paper, for example, manufactured in China, on which the books of that country are printed, is in many respects peculiar in its photographic properties, and in none more than in the comparative absence of the red and strength of the green tints produced on it, when subjected to the preparation of alternate washes of nitrate of silver and salt. The length, however, of the total spectrum is the same, but owing to the absence of the terminal red fringe, the darkened portion does not extend more than a semidiameter of the sun below the fiducial centre (thus marking a limit of action at that precise point), and the light portion is very feeble and indistinct.

Chemical Action traced much beyond the extreme Red Rays.—Exercise of a Blackening or deoxidating Power by the Red Rays themselves, under certain circumstances.

77. *Hydrobromate of Potash.* (Paper No. 571.)—Paper prepared with this salt instead of muriate of soda, gives a most extraordinary spectrum. The action commences, *the moment the rays fall upon it*, visibly over its whole length, and is uniform in intensity at every point except just at the extremities, where of course it fades gradually. But the most characteristic peculiarity of this spectrum is its extravagant length. Instead of terminating at the fiducial point, or thereabouts, *the darkened portion extends down to the very extremity of the visible red rays.* In tint it is pretty uniform (a grey-black not by any means intense) over the whole length, except that a slight fringe of redness (but no green or blue) is perceptible at the least refracted end. Beyond these evidences of deoxidation, thus traced fully up to the visible limit of the spectrum, it might be expected that all photographic action would terminate. Far otherwise; a contrary or oxidizing action now commences, marked by the maintenance of whiteness on the paper under the influence of dispersed light, and extends to a distance so considerable below the last traceable limit of the luminous spectrum, as to leave no possible room for mistake or doubt that chemical powers are exerted over a large extra-spectral region in this direction. The following statement of dimensions, a mean of three experiments, will place this important proposition in the clearest evidence. The measures are, as usual, from the fiducial point or centre of the yellow ray.

* See further on, Art. 77.

Extremity of the white portion	− 26·27
Sun's semidiameter	+ 3·60
Extremity of the chemical action in a linear spectrum or centre of the solar image in the extreme *chemical* ray	− 22·67
Centre of the solar image in the extreme *red* ray	− 13·30
Absolute extent of the chemical beyond the luminous spectrum	− 9·37
Commencement of the dull red fringe	− 15·05
Semidiameter of the sun	+ 3·60
Point in a linear spectrum to which the darkening influence would extend	− 11·45
Most refrangible extremity of the darkened portion	+ 90·50
Total length of the darkened portion	105·55
Entire length of the paper visibly affected	116·77

78. We may now observe, that this highly remarkable conclusion is also substantiated by the measurement stated in Art. 75. of the limit of whiteness on the muriated paper; for if from − 22·43, the coordinate of the termination of the sensible impression, we subduct the sun's semidiameter 3·6, we get − 18·83 for the limit of sensible chemical action on that paper in a linear spectrum * which already surpasses by − 5·5 the point (at − 13·3) at which the extreme red ray is situated in such a spectrum.

79. *Hydriodate of Potash.* (No. 577.)—The preparation of this paper is very variable in its results according to the strength of the solutions used. If strong solution of the hydriodate be used, it is nearly or quite insensible; if weak, the reverse. In the latter case the photographic spectrum it exhibits is not a little singular. It has the appearance of being inverted, the impression commencing very feebly and gradually at a point nearly coincident with the mean red ray of the luminous spectrum, where however the trace impressed is barely perceptible. Thence it gradually and regularly increases upwards till it attains its maximum of intensity at about + 50, that is to say, about five parts beyond the extreme violet. It then degrades more rapidly than it increased, till it vanishes at + 91·2, giving a total range of ninety-six parts. The tints are also remarkable. There is no imitation of the colours of the spectrum itself, but rather a kind of inversion of them. For the least refrangible end is of a dull ash grey or lead colour, and the most so, a considerably ruddy snuff-brown; the change of tint coming on rather suddenly about the end of the blue or beginning of the violet rays of the luminous spectrum.

Possible indication of Absorptive Action in the Sun's Atmosphere.

80. *Rochelle salt. Tartrate of Potash-and-soda.* (Papers Nos. 573, 574.).—Of all the salts I have examined, this presents, perhaps, the most singular peculiarities.

* By a linear spectrum I understand such a one as would be cast by a fixed star or other luminary of insensible angular diameter.

The quality of its papers however varies much with the doses of the ingredients, and, in fact, I have never succeeded in hitting again that precise proportion of the argentine and saline solutions which afforded the results I am about to state; and but that every portion of a pretty large sheet of paper agreed in giving the same, and that I have some of them now before me, I should hesitate to do so. The preparation of this particular specimen of paper, (marked 573–574 in my list of papers put on trial,) was, 1st, nitrate of silver S. G. 1·132; 2nd, saturated solution of Rochelle salt; 3rdly, nitrate of silver 1·132, which proving but little sensitive, a third wash of the nitrate was added, by which its sensibility was materially increased, its other qualities remaining unaffected. The paper was of the kind called "smooth wove demy."

81. The spectrum impressed on this paper commenced at, or a very little below, the fiducial point, of a delicate lead-colour, but faint and dilute, and when the action was arrested soon after the first impression made, such was the character of the whole photographic spectrum. But if the light was allowed to continue its action, there was observed to come on, suddenly, a new and much more intense impression of darkness, confined, in length, to the blue and violet rays, and, what is most remarkable, confined also *in breadth* to the middle of the sun's image, so far, at least, as to leave a border of the lead-coloured spectrum traceable not only round the clear and well-defined convexity of the dark interior spectrum, at the less refrangible end, but also laterally along both its edges. And this border was the more easily traced, and the less liable to be mistaken, by reason of its singular contrast of colour with the interior spectrum. That of the former, as observed above, was lead-grey: of the latter an extremely rich, deep, velvety brown. The less refrangible end of this interior brown spectrum presented a sharply terminated and regular elliptic contour, the more refrangible, a less decided one. The coordinates of its extremities may be stated at $+10{\cdot}0$ and $+48{\cdot}5$. Its length is therefore $38{\cdot}5$. Within this length occur two very distinct and evident maxima of intensity, viz. at $+21{\cdot}7$ and $36{\cdot}9$, the latter being by far the most conspicuous and abrupt. Another maximum also occurs beyond the limits of the dark-brown spectrum, viz. at $+56{\cdot}7$, in the grey or rather grey-brown colour, which the ground assumes at this end of the spectrum. The entire spectrum, including the lead-coloured portion at one end, and the grey-brown portion at the other, extends from $-3{\cdot}9$ to $+71{\cdot}5$, giving a total range of action $=75{\cdot}4$.

82. It may seem too hazardous to look for the cause of this very singular phenomenon in a real difference between the chemical agencies of those rays which issue from the central portion of the sun's disc and those which, emanating from its borders, have undergone the absorptive action of a much greater depth of its atmosphere; and yet I confess myself somewhat at a loss what other cause to assign for it. It must suffice however to have thrown out the hint; remarking only, that I have other, and I am disposed to think decisive evidence (which will find its place elsewhere,) of the existence of an absorptive solar atmosphere, extending beyond the luminous one.

The breadth of the border, I should observe, is small, not exceeding 0·5 or $\frac{1}{4}$th part of the sun's radius; and this, from the circumstances of the experiment, must necessarily err in excess.

Existence of a Blackening or deoxidating Power in rays BEYOND *the extreme Red.*

83. The tartrate of silver, whether *per se* or introduced into the pores of the paper by the processes above described, is by no means delicately sensitive. It remains long comparatively unaffected, and it is only when that peculiar action, above described, comes on, that the darkening process goes on with any degree of rapidity. Ultimately, however, it acquires a degree of blackness surpassing almost every other argentine compound tried. But there is a mode of accelerating this darkening and rendering the tartrated paper very sensitive, which has been already hinted at (Art. 48.), viz. by passing over it a wash of hydriodate of potash, which deserves more particular mention.

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84. If while a tartrated paper, prepared with two washes of nitrate of silver, and one, intermediate between them, of Rochelle salt, is exposed to the spectrum and in slow process of darkening, it be washed over with a considerably dilute solution of the hydriodic salt, it instantly begins to blacken much faster, not only over the whole positive or most refrangible portion of the spectrum, to which the action would have been limited without this application (Art. 81.), but over all the least refrangible or negative part, and *that, not merely up to the limit of the last visible* (extreme red) *rays, but considerably beyond that limit.* The spectrum produced, however, has nothing of that singular character described in Art. 81, but is of a pretty uniform brown over its whole length. Its measured dimensions are as follows:

Negative or least refrangible limit of darkness	− 22·5
Positive or most refrangible limit of darkness	+ 67·5
Total length of the dark impression	= 90·0

There are in this, as in the un-ioduretted paper, two conspicuous maxima of the solar action, the most abrupt, which shows like an intense oval spot of about fifteen parts (or twice the sun's apparent diameter) in length, has its centre at + 37, almost exactly where the more conspicuous maximum occurs in the un-ioduretted paper, but the other, which is less definite, takes place at or near the fiducial point itself. If from the negative coordinate − 22·5 of the last perceptible blackening we subtract − 3·6, the sun's semidiameter, we find − 18·9 for the point of extreme action in a linear spectrum, which surpasses by − 5·6 parts, or by nearly the whole distance which separates the mean from the extreme red, the last ray capable of exciting the retina. It ought also to be mentioned, that when the red rays are made to act on paper thus treated, and which has already undergone the darkening action of the blue or violet rays, they do not redden it, as in ordinary cases, which seems to indicate a difference in the nature of the chemical action exerted in the above experiment.

85. The evidence obtained in this experiment of the existence of a darkening or deoxidizing power in the invisible chemical rays beyond the extreme red, or (as perhaps we ought to express it) of the reversal, by the iodic salts, of the ordinary whitening or oxidizing power exerted by those rays (Art. 60.), is confirmed by measurement of the dark spectrum impressed on the paper (No. 644.) prepared with acetate of lead and chloride of platina when washed, under the action of the light, with hydriodate of potash, as described in Art. 47, as the following statement will show.

Negative sensible termination of the spectrum	− 21·5
Subtract sun's semidiameter	− 3·6
Last efficient ray in a linear spectrum	− 17·9
Last visible red ray in ditto	− 13·3
Extent of chemical action in ditto beyond the extreme red	− 4·6
Positive sensible termination of the spectrum	+ 70·0
Total extent of sensible chemical action	91·5

A power of precisely the same nature (i. e. of continuing the action up to and beyond the red rays,) is exercised by the hydriodate of potash also, on many other argentine compounds, as the oxalate, sulphate, chloride, &c., and probably on all.

86. The phenomenon of a triple maximum of chemical action observed in papers prepared with a tartaric salt, is also not peculiar to that class of bodies. In a paper (No. 578.) prepared with oxalate of ammonia, two very evident occur in the blue and at the extremity of the violet rays, and one, much less so, about the point where the green passes into blue. The spectrum is of a brown colour, of a dull and smoky cast at its less refracted end, but becoming livelier and more verging to a yellow at the other. *Its whole extent* lies clearly within the positive region of the solar spectrum, the sensible termination being at + 0·0, so that the place of the least refracted active ray in a linear spectrum would be + 3·6. The following are the measured coordinates.

Least refracted sensible termination	+ 0·0
Most refracted sensible termination	+ 88·0
1st. Maximum, barely traceable	+ 14
2nd. Ditto, pretty conspicuous	+ 25
3rd. Ditto, very conspicuous, and pretty abrupt	+ 41

In the spectrum exhibited on paper (No. 579.) prepared with citrate of ammonia, a triple maximum is also traceable.

Least refracted sensible termination	0·0
Most refracted sensible termination	+ 74·0
1st. Maximum	+ 24·7
2nd. Ditto	+ 39·4
3rd. Ditto	+ 56·9

The colours of the impression are smoke-grey or lead colour over the first twenty-four or twenty-five parts, and a purplish grey passing into a shade of dull pink over the more refracted portion.

87. Among the most remarkable spectra are some of those impressed on papers prepared with precipitates from liquids abounding in animal matter (such as are obtained by precipitating lead or other bodies from such liquids) applied as described in Art. 28. The paper described in the latter part of that article, when subjected to the solar spectrum, exhibited an impression whose coordinates and tints were as follows:

First sensible commencement of action at the least refracted end—*White*	− 23·0
White.	
Termination of white, commencement of faint brick red	− 5·0
Faint brick red.	
Termination of red, passing into a smoky brown	+ 1·0
Smoky dull brown.	
Commencement of a fine rich purple brown	+ 6·0
Gradually increasing in intensity to a maximum about	+ 26·0
Thence gradually declining, without change of tint, to the sensible termination of the impression at	+ 91·3
Consequently the total extent of chemical action on this paper, including the whitening or conservative action, which in this instance is marked with uncommon distinctness	= 114·3
Total length of the white portion	= 18·0

88. A precipitate consisting chiefly of phosphate of lime, but containing animal matter in combination, applied in mixture with a pretty strong solution of common salt, gave a spectrum not a little remarkable, being throughout of a uniform and very delicate lilac tint, with the slight exception of having the middle a little more verging to pink than the extremities. Its total length was ninety parts, commencing precisely at the fiducial point, and its maximum of intensity rather beyond the middle, towards the more refrangible end, viz. at + 60. When the same precipitate was applied intermediately between two washes of nitrate of silver, omitting the muriate of soda, a much more intense spectrum was produced, of a nearly uniform smoky brown or very dark grey, but peculiarly rich and velvety, barely attaining the fiducial point, and from thence, for a considerable distance, continuing extremely faint, and increasing by very gradual progress to a maximum about + 45, and decaying as gradually, but assuming at the more refrangible extremity a somewhat more ruddy hue, while at the less its tint inclined rather to a blueish or lead colour.

Effect of the Spectrum on certain Vegetable Colours.

89. The evidence we have obtained by the foregoing experiments of the existence of chemical actions of very different, and to a certain extent opposite characters at

the opposite extremities (or rather, as we ought to express it, in the opposite regions) of the spectrum, will naturally give rise to many interesting speculations and conclusions, of which those I am about to state will probably not be regarded as among the least so. We all know that colours of vegetable origin are usually considered to be destroyed and whitened by the continued action of light. The process, however, is too slow to be made the subject of any satisfactory series of experiments; and, in consequence, this subject, so interesting to the painter, the dyer, and the general artist, has been allowed to remain uninvestigated. As soon, however, as these evidences of a counterbalance of mutually opposing actions, in the elements of which the solar light consists, offered themselves to view, it occurred to me, as a reasonable subject of inquiry, whether this slow destruction of vegetable tints might not be due to the feeble amount of residual action outstanding after imperfect mutual compensation, in the ordinary way in which such colours are presented to light, i. e. to mixed rays. It appeared therefore to merit inquiry, whether such colours, subjected to the uncompensated action of the elementary rays of the spectrum, might not undergo changes differing both in kind and in degree from those which mixed light produces on them, and might not, moreover, by such changes indicate chemical properties in the rays themselves hitherto unknown. The want of sunshine has alone prevented me from pushing these inquiries to the extent to which, it will appear from the result of the only trials I have made, they well deserve to be pursued.

90. One of the most intense and beautiful of the vegetable blues is that yielded by the blue petals of the dark velvety varieties of the common Heartsease (*Viola tricolor*). It is best extracted by alcohol. The alcoholic tincture so obtained, after a few days keeping in a stoppered phial, loses its fine blue colour, and changes to a pallid brownish red, like that of port wine discoloured by age. When spread on paper it hardly tinges it at first, and might be supposed to have lost all colouring virtue, but that a few drops of very dilute sulphuric acid sprinkled over it, indicate, by the beautiful and intense rose colour developed where they fall, the continued existence of the colouring principle. As the paper so moistened with the tincture dries, however, the original blue colour begins to appear, and when quite dry is full and rich. The tincture by long keeping loses this quality, which does not seem capable of being restored. But the paper preserves its colour well, and is even rather remarkable among vegetable colours for its permanence in the dark, or in common daylight.

91. A paper so tinged (No. 599.), of a very fine and full blue colour, was exposed to the solar spectrum concentrated, as usual, (October 11, 1839,) by a prism and lens; a water-prism, however, was used in this experiment, to command as large an area of sunbeam as possible. The sun was poor and desultory; nevertheless in half an hour there was an evident commencement of whitening from the fiducial yellow to the mean red. In two hours and a half, the sunshine continuing very much interrupted by clouds, the effect was marked by a considerable white patch extending from the extreme red to the end of the violet, but not traceable beyond that limit.

Its commencement and termination were, however, very feeble, graduating off insensibly; but at the maximum, which occurred a little below the fiducial point, (corresponding nearly with the orange rays of the luminous spectrum,) the blue colour was completely discharged. Beyond the violet there was no indication of increase of colour, or of any other action. I do not find that this paper is discoloured by mere radiant heat unaccompanied with light.

92. Paper washed with an alcoholic solution of guaiacum was exposed to the spectrum formed by the FRAUNHOFER flint prism, and concentrated as usual. The paper before exposure was of a very pale yellowish colour. The action was slow, (though much quicker than in the case of the tincture of heartsease,) and extended from + 4·7, or thereabouts, to + 72·9, attaining its maximum at + 58·5, or beyond the violet, far in upon the lavender rays. The impressed spectrum throughout its whole length (68·2 parts) was of a fine celestial blue colour, though of little intensity.

93. A slip of the same paper was exposed for some time to sunshine, defended from the action of all but the blue and violet rays by cupro-sulphate of ammonia, acquiring thereby a uniform pale blue tint, verging to greenish. It was then exposed to the spectrum as in the last article. The effect was highly remarkable: the impression produced consisted of two long very faint streaks, the one, corresponding to the more refrangible part of the spectrum, from about + 42 to + 68, was of a blue colour, darker than the general ground, and having its maximum at nearly the same point as in the last experiment, viz. at + 58. The other, corresponding to the comparatively less refrangible portion of the total spectrum, commenced above at about + 27, as nearly as the extreme faintness would enable the eye to seize its beginning, and extended downwards to − 16. In this portion the blue colour of the ground of the paper was discharged more or less completely, and most so (where the original pale yellow colour of the paper was quite restored,) at − 3, that is to say upon the orange ray, and *by no means at that point of the spectrum where the caloric effect is greatest**. The effect therefore cannot be due to mere radiant heat. The neutral part from + 27 to + 42, embraces the whole of the violet and nearly all the indigo rays of the luminous spectrum.

V. *Of the Whitening Power of the several Rays of the Spectrum under the influence of the Hydriodic Salts on Papers variously prepared and previously darkened by the action of Solar Light.*

94. The singular effect of a moderately strong application of the hydriodate of potash to darkened photographic papers, in rendering them susceptible of being whitened by further exposure to light, has been already alluded to. But the prismatic analysis of this effect, as exhibited on various preparations of silver, offers a series of new relations so very curious, as may well require the bestowal on them of

* See the Notes at the end of this paper.

a few paragraphs descriptive of such very imperfect experiments as I have hitherto been enabled to make on this subject.

95. A paper (No. 646.) prepared with acetate of lead, hydrobromate of potash and nitrate of silver, when exposed, without any further preparation, to the spectrum, exhibited an instant action over the whole spectrum and down to the extreme red, beyond which appeared (the paper having been somewhat discoloured by keeping) a white circular spot to the extent of a full diameter of the sun, which though not strongly marked was unequivocal. A specimen of the same paper previously darkened to a considerable degree by exposure to sunshine, was then subjected to the action of the spectrum, and while so subjected was washed with a dilute solution of hydriodate of potash. It speedily began to *whiten* at the more refrangible end of the spectrum, the whitening commencing in the violet rays; but so far from their action being continued downwards into the less refrangible region, the paper in that region *continued to blacken* perceptibly, *and the blackening extended even below the extreme red**. A second wash of the iodic solution being now added till the paper was drenched with it, the negative or darkening action was found to be arrested in the part of the paper already acted on, while on presenting a fresh portion of the surface so drenched to the spectrum, no such action took place.

96. Similar experiments were made with various other preparations of paper, both with and without the use of lead; such as with borax, muriates of baryta, strontia and lime, sulphate and phosphate of soda, &c. It will only be necessary to detail particulars in a few cases offering peculiarities. The general mode of action seemed to be as follows. 1st. When the darkening by previous exposure to sun has not gone too far, a weak wash of the liquid hydriodate brings the paper into a sort of intermediate state, in which it continues to be further blackened, or, according to our nomenclature, has a negative character, under the action of the less refrangible rays; and that, quite down to the red end of the spectrum, and even below it in cases where, without such addition, the darkening would have terminated at the yellow ray. On the other hand, under the more refrangible rays it merely ceases to be darkened, losing its negative without acquiring a positive character. When the hydriodic application is somewhat more copious, the paper becomes under these rays distinctly positive, whitening slowly and gradually under their action, while between the darkened and whitened portions a neutral interval occurs. If a further dose of the hydriodate be applied, the neutral line descends in the spectrum, the darkening in the less refrangible portion is enfeebled, and the whitening in the more refrangible reinforced, but by no means equally. On the contrary, it not unfrequently happens that a tolerably definite portion of the spectrum, viz. just where the blue and violet join, is marked by a much more intense whitening action than that beyond it, so as to terminate the whitened portion by a conspicuous white spot. If still more of the hydriodic solution be added, or if it be applied stronger, all blackening ceases in every part of the spec-

* See Art. 85.

trum, and a white spectrum is produced, extending in some cases (but always feebly) below the fiducial point, having its maximum of intensity at the junction of the blue and violet, and fading away gradually over the more refrangible region. If the dose be very strong, the whitening goes on rapidly even without the aid of light.

97. A paper prepared with acetate of lead and then chloride of platina (neutralized by lime) was washed over, not with the pure hydriodate of potash, but with a mixed solution of this salt and tartrate of soda and potash. The action of the hydriodate was enfeebled to a greater degree than by the mere effect of dilution. It required to be excited by a second application of the liquid, and even then the darkened space, in lieu of extending to the negative extremity of the spectrum, broke off at the fiducial point. On applying, however, a third dose, the action was again excited and carried instantly down to and far beyond the extreme red. It has been already noticed (Art. 47.) that paper so prepared is insensible *per se*, and acquires a negative character under the influence of the hydriodic salts, the *gradual coming on of which character* for rays of less and less refrangibility as the dose of the saline application increases, it is the object of this experiment to trace.

98. In some instances, where, under the influence of the hydriodate, the paper has been rendered positive (i. e. whitened by light) for the violet and ultra-violet rays, and negative for the rest of the spectrum down to the red, another change of character from negative to positive has been observed to take place beyond the red, indicated by a slight but unequivocal whiteness produced about the region of the extreme red. This singular alternation of character, it may be as well to mention, occurred in two papers (Nos. 649, 656.), the one prepared with phosphate of soda, the other with muriate of strontia, both with a mordant base of lead; but it is probably not so much dependent on the peculiar preparation of the paper as on some accidental adjustment of the dose of the iodic salt in the experiments referred to.

99. In one experiment only was any other colour than mere black or dark brown, and white (or rather pale greyish yellow), observed to be produced under the influence of the hydriodic application. The paper in this case (No. 672.) had been prepared with muriate of lime and nitrate of silver without a mordant wash, and, when darkened by previous exposure and then subjected to the joint action of the spectrum and the iodic solution, was found to be negative in its character for rays more refrangible than the yellow, while in the less refrangible region it was strongly reddened. It should be observed, however, that in paper so prepared, and without the hydriodic application, the simple action of the spectrum developes an unusually strong red at the point appropriate to that colour.

VI. *Of the Analysis of the Chemical Rays of the Spectrum by Absorbent Media.*

100. As the effectual study of mixed colours, as nature presents them to us, would be impracticable by the mere use of absorbent media, unaided by prismatic analysis, so that of the photographic relations either of argentine or any other class of re-agents

would be equally unavailing, were such media alone resorted to for the purpose of effecting a separation of the luminous elements prior to their action. A combination of their analysis with that effected by the prism will lead, however, to results of no small interest. The facts I am about to recite, so far from exhausting or even systematically arranging and attacking the subject, are hardly more than sufficient to show what a singularly wide field of inquiry is here laid open to us.

101. To study this subject in its most simple form, it would be necessary to operate on some preparation which shall be equally and indifferently sensitive to every ray of the spectrum. Such a preparation, with exception of the rays beyond the extreme red, exists in the bromuretted paper of Art. 77. But when the paper used to receive the spectrum, after undergoing absorptive analysis, has its own peculiar law of sensitiveness, it is evident that the resulting impression left on such paper will be a mixed effect depending on two distinct functions, the one expressing the degree of transmissibility or *diacratic* index of the medium for that ray, the other that of sensibility in the paper to the action of such ray.

102. I have already stated that I saw no reason from experiments made with prisms of water, crown- and flint-glass, to suspect any considerable difference in those media as to their respective scales of diacratescence*; but it would be hasty on the strength of those experiments to affirm that none exists, and quite unwarranted to extend the conclusion to other colourless media. On such it was my design to have instituted a series of experiments expressly directed to this object; but the want of sun rendered it impracticable, since it is useless, or only tends to confuse and mislead, to pursue such inquiries without the use of the spectrum; and the various and striking phenomena exhibited by *coloured* media naturally gave them a precedence in point of interest. Some of the more remarkable of those which have fallen under my observation I shall now proceed to describe.

103. Red, brown, and yellow media are well known to exert their greatest absorbent energy on the more refrangible rays; and the progress from the palest yellow to the deepest red is marked by a corresponding disappearance in the spectrum, as transmitted by or viewed through such media, of these rays progressively, commencing with the violet and proceeding downward through the spectrum till only the extreme red is left. The various shades of brown seem to depend chiefly on the proportions in which the green rays are attacked in the progress of this general destruction. Nothing, however, which the eye can seize in the character of such media, in the ordinary mode of examination, indicates any tendency towards an increase of transmissibility in the ultimate violet, or anything to authorize a suspicion that rays beyond the violet would not be cut off by such media, even more energetically than the visible violet rays themselves. It is true that by those red media, whose tint verges to a rose colour or pink, both blue and indigo rays are transmitted pretty copiously; but the

* Perhaps *diacrasy* would be preferable to a term confusing, as diacratescence does, a Greek and Latin origin.

addition of a pale yellow glass to such a medium eliminates these, and converts the combination into a pure red. But the photographic character of brown and yellow media is far from conforming itself *universally* to this indication. Of this the common yellow and brown glasses which are met with in the shops afford a striking example. The spectra impressed by transmission through such glasses consist of two distinct portions, separated by a larger or shorter interval, and having themselves a less or greater extent and intensity in proportion to the depth of tint of the glass. If that tint be only a pale yellow or straw colour, both portions are finely developed. If in that case the exposure be long continued, the continuity of the spectrum is maintained by a feeble train of darkness in their interval. Abstraction made of this, the most refrangible portion may be stated to commence precisely at the end of the violet, or at + 40·6, whence it increases gradually to a maximum at + 60, and thence dies gradually away, till it ceases to be traceable at + 85, giving a range of forty-four parts in the ultra-violet rays for its extent. The interval between the portions measured in the same experiment from which the foregoing numbers are taken, and a similar abstraction made of the faint connecting train of darkness, was twenty-three parts, occupying the whole space, which in a complete spectrum would have been filled with violet and indigo light. The least refracted portion, commencing at + 18, rapidly attained the full intensity it would have had if freely exposed without the yellow glass, and from thence to − 11, where it terminated, offered the same successive tints (dark blue, green, and red, the paper having been prepared with muriate of soda) which it would have done in those circumstances. As to the tint of the ultra-violet portion, it was a fine violet-purple without any variation.

104. When the colour of the glass used was a pretty strong brown, such as to cut off all the violet, indigo, blue, and the greater part of the green, the ultra-violet portion of the photographic spectrum, though much enfeebled and contracted, was still distinctly traceable from + 58 to + 83, the maximum (at + 70) being considerably more remote than in the former case, and this might be owing to an actual difference in the colouring matter of the glasses. On the other hand, the less refrangible portion had shrunk to a small and well-defined oval spot, extending from + 3·5 to − 9·5, or over thirteen parts, or rather less than two diameters of the sun, having its upper part coloured lead-grey, the lower red.

105. One of the finest and most brilliant yellow liquids with which I am acquainted for purposes of absorptive analysis, is the solution of that substance to which Dalton has given the name of quadro-sulphuret of lime, and which is easily prepared by boiling lime and sulphur together in plenty of water. Its transparency is so great and its tint so luminous a yellow, that it scarcely seems to impair the brightness of white objects seen through it in thicknesses of an inch or two. But its action on the more refrangible end of the spectrum is very energetic, and at a thickness = 1·1 inch was found to be such as to limit the visible spectrum to an extent of 33·2, including the two terminal semidiameters of the sun. Only a faint trace of blue was visible at the

more refrangible end. And to these indications the photographic spectrum was found to correspond, being reduced to an oval patch not exceeding nineteen parts in extent, between the limits + 13·5 and − 5·5, and that, it should be observed, of a uniform tint, the red fringe at the least refracted end (which was pretty conspicuous on the paper used when subjected to the unimpeded spectrum) being deficient, from which it may be surmised that this liquid acts more powerfully on the chemical rays at the red end of the spectrum than on the luminous. The contrast between the complete and absorbed spectra in this experiment was indeed most striking, considering the small apparent loss of illuminative power. The unabsorbed spectrum extended from − 8·0 to + 87, or over ninety-five parts, throughout fully sixty-five of which its intensity was uniform, and its hue deep black. A like degree of blackness extended only over thirteen parts of the absorbed spectrum, so that while the illuminating power of an incident sunbeam was so little impaired by the action of the liquid as already stated, its total photographic effect must have been diminished by the loss of at least four fifths of its amount. It should also be mentioned that in this experiment there was not the least trace of the action being resumed in the region beyond the violet rays.

106. The singular properties of nitrous gas pointed out by Sir David Brewster as exhibited in its absorbent action on the spectrum, which it breaks up through its whole extent into alternating bright and dark spaces, led me to expect some equally singular affection of the photographic spectrum after undergoing its absorption. But this expectation was not fulfilled. The photographic impression was reduced by the action of a glass globe filled with this gas, to a small extent (twenty-five parts) by the destruction of all action beyond the blue rays; but in what remained I could discern no trace of interruptions of continuity. The experiment, I should observe, however, was difficult, having no means of drying the gas at hand, so that to prevent the deposition of moisture the globe was obliged to be frequently heated. It would be desirable therefore that it should be repeated, and that other absorbent gases should also be tried.

107. Another and no less striking example of the independence of the laws of absorptive action of media as exerted on the chemical and luminous rays, is afforded by the cupro-sulphate of ammonia. That beautiful liquid, as I have shown in my paper above referred to*, acts with the greatest energy on the red end of the spectrum, which it completely obliterates in a very small thickness, the action diminishing progressively till it seems reduced almost to nothing at the more refrangible extremity. Its scale of diacratescence, however, is quite different from what this indication would lead us to expect. The absorptive action on the chemical, as on the luminous rays indeed, at the less refrangible end, is intense, and goes on diminishing progressively to about the middle of the indigo, but here it begins again to be reinforced, and attains an energy in the region of the extreme violet, and over the whole extent of the

* Transactions of the Royal Society of Edinburgh, 1822.

yet more refracted rays, sufficient to destroy all photographic action over that part of the spectrum, and to confine its effective range between the limit of the green and blue rays on one hand, and the extreme violet on the other.

108. A green glass limits the photographic spectrum to a short oval patch extending from the fiducial point, or a very little below it, to + 18 or 20, in which no coloured fringe can be traced. The glass used was of a pure grass green hue of considerable intensity, neither verging to the yellow on the one hand nor to a blue on the other. When sulphate of nickel was used, the impression was of somewhat greater extent in both directions, viz. from about − 3 to + 28; but in both cases the action of the indigo and violet rays, and of all beyond them, was totally cut off.

109. Among dichroïte media (or those which in some certain thickness insulate two portions of the luminous spectrum, or which are much more eminently transparent for two rays than for all the rest,) there is none more remarkable than the muriate of chromium*, which reduces the spectrum to two narrow and pretty well defined spaces, coloured, the one red and the other green, the red being that at the extremity of the spectrum, and the green of great purity and richness of tint. Nor is the action of the spectrum thus analysed on photographic papers less remarkable. On such paper (prepared with common salt) it impresses, under a considerable thickness of the medium, two circular spots, the one intensely black, the other white, the places of whose centres coincide with those of the coloured images, exactly in the green, and nearly so in the red, only somewhat lower, indicating the action of invisible rays. When the liquid is more dilute, or the thickness diminished, so as to render the coloured images perceptibly oval, these spots, in like manner, extend into ovals, and moreover, a third, much fainter, makes its appearance in the region beyond the violet, where no perceptible illumination falls. As the liquid is more diluted, or its thickness more reduced, this spot gains strength, and connects itself by a faint train of darkness with the much more intense one corresponding to the green image. In like manner also may the cobalt-blue glass already so often spoken of, which is in effect a polychroïte medium, be made to insulate a definite circular white spot in the photographic impression of its spectrum, corresponding to those extreme red rays which it insulates in such perfection. To return, however, to our chromic solution It is impossible not to be struck with the much greater energy of chemical action in the green which this medium insulates, than with that insulated by the green glass already alluded to; and the following experiment presents us with a green of considerable illuminating power, which is almost or quite devoid of chemical action.

* Vide article Light, Encyclopædia Metropolitana, under the head of Absorption, where the curious properties of this medium were, I believe, first pointed out. At that time I regarded them as unique, for the dichroïte action of sap-green is not nearly so definite; but I have since found the same property in two other liquids, viz. in a sulphuric solution of sublimed indigo diluted with alcohol, and in a sort of ink sold under the name of Stephen's Writing Fluid: the green in this last, however, is less pure and definite; and it differs also from the other two in extinguishing the red with more energy than the green, in consequence of which its ultimate tint is green, while that of the others is red.

110. The colouring matter of archil is in many respects highly remarkable. It may be insulated by precipitating the ammoniacal liquid, commonly sold under that name, by dilute sulphuric acid, washing the insoluble matter which falls, redissolving in very dilute carbonate of soda, and again precipitating. It is very sparingly soluble, if at all, in water, but most readily so in all alkalis, which take up an enormous quantity, and afford solutions of the most intense conceivable purple colour. To this colouring principle, which, like others of the same class*, has many of the characters of a weak acid, the name *Rocellic acid* (*Lichen Rocellus*) may be not inappropriately applied. The rocellate of potash, greatly diluted, transmits a spectrum which may be considered as typical of those afforded by purple media, the yellow ray being attacked with peculiar energy, and entirely absorbed by a very small thickness or density of the medium. When a spectrum so transmitted was received on muriated photographic paper, it was curtailed both at the most and least refrangible end, and reduced in its extent to the space included between the coordinates + 8 and + 72. Allowing therefore 3·6 for the sun's semidiameter, it appears that the rays in the linear spectrum as far as + 11·6 from the fiducial points, were rendered inactive. Yet so far from the luminous rays being extinguished in this interval, so dilute was the solution used, that a great quantity of green light occupied the undarkened space in question, such as, had its properties not been altered by the medium it had traversed, could not have failed in the time the exposure lasted, to have produced a considerable blackening of the paper. That produced by the *unabsorbed* spectrum over the same region was very intense, as is always the case with muriated preparations of paper.

111. The faint spot beyond the violet produced in the spectrum which has undergone the action of the muriate of chrome, may possibly have some direct connexion with that peculiar illuminating ray to which the epithet lavender has (whether properly or not) been applied in the foregoing pages. At least I find that, on viewing the oval spot of that light (concentrated as described in the earlier part of this paper by a prism and inclined lens) through various coloured glasses, it is not absorbed by either green or yellow glasses sufficiently deep to obliterate the *whole of the violet*; and, what is extremely remarkable, such glasses, by suppressing in a most decisive manner the violet illumination in this part of the spectrum, and by defending the eye from extraneous light, reveal the existence of a faint but evident substratum (so to speak) of unabsorbed light, of a similar character, connecting the spot in question with the rest of the spectrum; unless, indeed, which is not impossible, this be merely

* Such as the colouring principle of the common Heartsease, which, as the experiment described in Art. 90. proves, has, like that of archil, a deoxidated form; that also which abounds in the black ebony wood, and which is easily extracted from it by decoction in water, and precipitation by nitric acid, &c. I may be allowed to mention that the name, and the chemical process described in the text, are taken from notes of experiments made on archil in September, October, and November 1829, while ignorant that the subject had engaged the attention of Robiquet, whose elaborate memoir on that Lichen appeared in the Annales de Chimie about that time, thereby preventing the publication of my experiments; some notice of which, however, by M. Quetelet, appeared in the Journal des Sciences de l'Acad. Royale de Bruxelles, (Seance du 22 Mai, 1830.).

the most refracted extremity of the spectrum doubled back on itself. On the other hand, this spot *is* absorbed by cupro-sulphate of ammonia, which also attacks the green, as well as by a rose-coloured glass, which lets the blue rays pass in abundance, but destroys both the green and violet.

112. In further illustration of the action of coloured media, I may mention here some very curious results which have been communicated to me by Mr. HUNT above mentioned, and which I have his permission to insert. They seem to indicate some peculiar action of the barytic salts, but the habitudes of the media employed with relation to the pure prismatic rays not being stated, it is impossible to refer them to their precise origin in the spectrum.

"A paper," says Mr. HUNT, "prepared by washing with muriate of barytes and nitrate of silver, allowed to darken whilst wet in the sunshine to a chocolate colour, was placed under a frame containing a red, a yellow, a green, and a blue glass. After a week's exposure to diffused light, it became bright red under the red glass, a dirty yellow under the yellow, a dark green under the green, and a light olive under the blue.

"The above paper, washed with a solution of a salt of iodine, is very sensitive to light, and gives a beautiful picture. A picture thus taken was placed beneath the above four glasses, and another beneath four flat bottles containing coloured fluids. In a few days, under the red glass and fluid, the picture became a dark blue, under the yellow a light blue, under the green it remained unchanged, whilst under the blue it became a rose red; and now, after three weeks, this red is slowly changing to green.

"I took a copy of an engraving under the glasses with the same paper. There was no action beneath the red glass, not much beneath the yellow, but beneath the blue it was very perfect, and, strange to say, beneath the green glass every line was faithfully copied; but the dark parts of the engraving were the brightest of the copy, as if it had been taken on one of the white photogenic papers."

VII. *Of the exalting and depressing Power exercised by certain Media, under peculiar circumstances of Exposure to Solar Light, on the Intensity of its Chemical Action.*

113. When thin post-paper, merely washed with nitrate of silver, without any previous or subsequent application, is exposed to a clear sunshine, partly covered by and strongly pressed into contact with glass, and partly projecting beyond it, so as to be freely exposed to air, the darkening produced in a given time is very unequal in the two portions. That protected by the glass, contrary to what might have been expected, is very much more affected than the part exposed; more, indeed, in some instances, than would be produced by free exposure during three or four times the given time. When fixed by hyposulphite of soda, the difference is rendered yet more striking, to an extent hardly credible without trial.

114. This singular effect was observed at the very outset of my inquiries, and was

noticed in my first communication to the Royal Society. It was attributed at first successively to a variety of causes, such as retention of aqueous vapour, reflection backwards and forwards of the incident light between the surfaces of the glass and paper, to a development of heat at the point of contact, disengagement of gas, &c. None of these causes, however, appear adequate to account for the observed effect. Mere moisture has but little influence on argentine papers. The additional light returned on the paper by continued reflection is much too small, and this explanation is opposed by the observed fact, that the effect in question is imperceptible in a gloomy day, however long the exposure be continued, and that it increases in degree as the sun is stronger. This would certainly favour the idea that the cause is to be sought in heat developed and retained by the glass; and this explanation may appear to be supported by the fact, that the effect in question is not produced when the glass, instead of being pressed into contact with the paper, is merely placed before it as a screen, with a considerable interval between them. When a piece of nitrated paper, for instance, was rolled round a cylindrical surface of moderate convexity, covered with black velvet, and the glass gently laid in contact with it, the effect of sunshine was exalted at the line of contact; but on either side of that line, as the interval increased, the influence of the glass diminished, and at less than half an inch distance no difference could be perceived between the impressions under the glass and in free air. Nevertheless, on trying to produce the same effect in gloomy days, by heat alone applied to a metal plate in contact with the paper, no increase of action was observed unless the heat was fully that of boiling water. But this is far greater than can be acquired, under the circumstances of the experiment, by the mere action of the sun. Moreover, I find that the same exalting effect is produced when a thin close-textured white paper is interposed between the nitrated paper and the glass, and *that* when the sun has far from its full power; under which circumstances only a very moderate warmth can be excited at the point of action.

115. *When first this phenomenon was noticed, the idea was suggested of two opposing influences in the solar light, the *difference of which only* becomes sensible in the immediate chemical effect, and of which the negative or deoxidizing one (using the term in a mere conventional sense) is partly stopped by the glass. In pursuance of this notion, red light, concentrated by a lens, after reflexion at the surface of a looking-glass, was thrown on paper exposed to the free sunshine, but without any perceptible effect in retarding its discoloration.

116. In further prosecution of this subject I fastened (March 24, 1839,) on a piece of nitrated paper, colourless, or nearly colourless plates, of the following substances:

* This paragraph is copied verbatim from my first communication on this subject. The paper used was nitrated, the sun feeble, and all circumstances unfavourable, (February 10, 1839). Had muriated paper been used, however, the experiment so conducted could have hardly failed to have verified (at least so far as it goes) the peculiar views of solar action which suggested it, and thus to have led to the earlier discovery of those singular properties of the less refrangible rays described in the foregoing part of this paper.

1. Saxon topaz, parallel to the axis.
2. Saxon topaz, perpendicular to the axis.

} These plates had a very slight purplish tinge.

3. Sulphate of lime.
4. Iceland spar, a natural rhomboid.
5. Plate glass, slightly tinged with blueish green.
6. Rochelle salt, parallel to the axis (not very well polished).
7. Thick quartz, perpendicular to the axis.
8. Thin quartz, perpendicular to the axis.
9. Thin quartz, oblique to the axis.
10. Thin quartz, differently oblique to the axis.

After exposure to the sun for a sufficient length of time to darken the ground of the paper considerably, it was found on taking off the plates and fixing the whole with hyposulphite of soda, that all the substances employed had considerably exalted the solar action, though not all in the same degree. The plate-glass, which, as observed, was a little coloured with blueish green, had produced decidedly the greatest effect, the discoloration beneath it being at least three or four times that of the general ground of the paper. The order of the others was as follows:

Nos. 7, 4 . . . . 6, 8, 3, 10 . . . . 9, 2, 1,

which, on the whole, was nearly that of the thicknesses of the plates, and does not seem, except in the case of the glass, to indicate any *peculiar* action in the media; and in this case the slight tinge of green, by stopping the chemical rays beyond the red†, may be supposed to have increased the action.

117. At the same time that this experiment was made another circumstance was noticed, which added to the obscurity of the subject. A specimen of paper, infinitely more sensitive than the nitrated paper I at that time used, was sent me by Mr. Talbot. This paper, treated exactly in the same manner, and exposed at the same time with a specimen of the nitrated paper, exhibited no such exalting effect. This induced me to subject, at various periods, to similar trials a great number of preparations and several varieties of paper, the results of which appeared exceedingly capricious, as in some cases it was found that exactly the reverse effect was produced, and that the superposition of one and the same glass in several instances exercised *quite as remarkable an influence in depressing, as in others it did in exalting the solar action*; while in others, again, it seemed to exercise no influence at all. The quality of the paper, too, appeared to exercise a marked influence on the result. And differences in the mode of preparation, and the doses of the ingredients, too minute to be noted, would alter materially the degree, and sometimes even the character of the effect. The following Table contains several of the results of these trials. Column 1. contains the number of the paper; column 2. the character of the effect, in which + indicates an exalting influence on the glass, 0 indifference, and − a depressing in-

† See Art. 77.

fluence; repetition of the sign indicating a greater intensity of the observed effect, whether exalting or depressing. The third column gives the characteristic ingredient of the chemical preparation used for rendering the paper sensitive, and the fourth the description of paper employed.

No. of Paper.	Character of Effect.	Characteristic Ingredients of Preparation.	Sort of Paper.
12	+ + +	Simple nitrate of silver	Thin post, of very even texture.
694	+ +	Ditto	Unsized white paper.
570	0	Ditto	Smooth demy.
583	+ + +	Borax	Smooth thick wove post.
614	0	Ditto	Smooth demy.
593	+ +	Tartrate of antimony	Thick wove post.
598	+ + +	Rochelle salt	Smooth demy,
582	+	Phosphate of soda	Thick wove post.
580	+	Hydrocyanic acid	Smooth demy.
581	+	Bicarbonate of soda	Thick wove post.
571	0	Hydrobromate of potash	Smooth demy.
	0	Unknown, but supposed hydrobromate of potash	Specimen prepared by Mr. Talbot.
545	− −	Muriate of soda	Thick wove post.
550	−	Ditto	Thin post.
547	0	Ditto	Ditto.
556	+	Ditto	Smooth hotpressed note-paper.
584	−	Ditto	Chinese paper.
603	+	Ditto	Thick post, a different sort from 545.
607	0	Ditto	Smooth demy.
623	− − −	Ditto	Blue wove post.
579	−	Citrate of ammonia	Smooth demy.
578	− −	Oxalate of ammonia	Ditto.
609	− − −	Muriate of soda over acetate of lead	Ditto.
610	− − −	Muriate of soda over nitrate of lead	Ditto.
613	− − −	Phosphate of lime precipitated from solution of nitrate of urea	Ditto.

Whether these facts have any connexion with the properties of colourless liquids discovered by M. Malaguti, I am unable to conjecture, my knowledge of his experiments being derived merely from verbal report.

VIII. *Description of an Actinograph, or self-registering Photometer for Meteorological purposes.*

118. As I do not mean this paper for a regular treatise, as on the contrary it pretends only to be a mere collection of insulated facts, observations, and processes, I may be pardoned for inserting in this place an application which very early suggested itself, and which, as far as I have tried it, proved satisfactory in its working, viz. that of a self-registering meteorological photometer or *actinograph*. The objects of such an instrument, which cannot but be one of material importance to the meteorologist, the botanist, and the general physiologist, may be considered as two-fold, viz. 1st, to obtain a permanent and, at least, self-comparable register and measure of the momentary amount of general illumination in the visible hemisphere which constitutes daylight; and 2ndly, to obtain a similar register of the intensity, duration,

and interruption of actual sunshine, or, when the sun is not visible, of the illumination of that point in the clouded sky behind which the sun is situated.

119. To accomplish the first of these objects, it would be necessary, in strictness, to expose the photographic paper destined to receive the register, to a full uninterrupted view of every part of the sky, that occupied by the sun's disc alone excepted, which would not be very easy to accomplish, without a somewhat complicated apparatus, to cut off the direct solar rays, by a moving screen of the requisite diameter. But if we consent to sacrifice the light of the zone of the heavens traversed by the sun in its diurnal motion, and such other small portions as may be intercepted by a slight frame-work, nothing is more easy. Let a weather-proof box or case be constructed, within which a clock-motion shall carry round, once in twenty-four hours, a cylindrical barrel having its axis horizontal, and its upper surface close to the under surface of the top of the box, which at this point must be thin, and pierced with a narrow oblong window bevilled off at the edges so as to admit light from every part of the sky (but what is intended to be screened) to fall on that part of the circumference of the cylinder which travels close beneath it. The cylinder then being daily covered with a strip of sensitive paper, will of course register the total illumination which the slit admits to fall upon the paper, that is to say, the light of the hemisphere *plus* the direct sunshine if the latter be not intercepted. To accomplish this nothing further is needed than to adjust a light cylindrical hoop of brass to slide smoothly but stiffly up and down along three straight, parallel steel or brass rods, of no greater diameter than is necessary to give the requisite strength; the direction of which shall be towards the pole of the heavens, and whose places shall be such that the axis of the hoop shall pass through the middle of the exposed area of paper. The upper ends of these rods should terminate in and be connected by a strong brass ring sustained by a vertical pillar; the lower must be fixed to the pedestal or other support of the clock and its case, on which also the pillar above mentioned must be fixed. It is evident then that at any time of the year, or whatever be the sun's declination, if the hoop be adjusted and clamped so as to throw its shadow across the window at one moment of the day, it will do so throughout the whole day, and the proper place for each day in the year may be marked on the rods.

120. This contrivance, though convenient, and necessarily effectual, is not that which I ultimately adopted, which is as follows. A spring watch belonging to a 'Sgravesande's heliostat (as constructed by Robinson) has a hollow axis for the hour-index passing completely through it, from face to back, so as to permit a steel axis, removable at pleasure, to traverse it, and issue below. It is mounted on a stand so as to admit of adjustment both in altitude and azimuth, the former by a small divided quadrant and vernier, and so placed (according to the intention of its original use) as to have the upper surface or dial-plate of the watch parallel to the equator, and of course the axis of the hour-index directed to the pole. On this axis, and at the upper surface of the watch, was fastened a disc of sensitive paper, attached to a

light circular card (to keep it flat) by slight touches of soft cement. This appendage was brought as close as possible to the dial-plate, avoiding contact, and was of course carried round uniformly once in twenty-four hours by the rotation of the axis. Over this a shade or cover was adapted, having an aperture in the form of a very narrow sector of the circle, to admit the light. If the paper be as sensitive as it ought, the smallest chink suffices, an amplitude of a degree in angular subtense at the centre being more than enough. The edges of the aperture should be so bevilled, or otherwise defended by screens, as to admit no direct ray of the sun when north of the equator, nor any other light than what emanates from that definite circumpolar region of the sky to which it may be considered desirable to limit the observation, such as, for example, a circle whose semidiameter shall equal the latitude of the place in extratropical countries; unless we prefer to register the actual illumination of the sky at the pole, in which case a tubular screen pointed to the pole must be adapted to the whole apparatus. Under these circumstances, we shall find at the end of each day the changes of illumination registered on the sensitive paper by a fan-shaped alternation of light and shade, radiating from the centre over so much of the area of the circular disc of paper as corresponds to daylight, or to such twilight as the paper is sensitive enough to be discoloured by. Of course it will be recollected in reading this description that the pole is the only point in the hemisphere whose illumination is independent of the sun's hour-angle, and which can therefore properly be taken as likely to afford an average of the clear or cloudy state of the sky.

121. For registering the *direct* effect of solar radiation it is necessary, 1st, that the paper should at every moment be presented at right angles, or nearly so, to the incident beam. The greatest inclination of a sunbeam, however, not exceeding $23\frac{1}{2}$ degrees to the equator, a paper wound on a cylinder having its axis pointed to the pole will always fulfil this condition near enough, the difference between the cosine of that angle and radius being only eight per cent.; and the error being, moreover, one susceptible of allowance by a trifling calculation, or by a table, being the same for the same day in every year. Moreover, as the sun travels round in its diurnal course, and is required to throw a limited beam in succession on every part of the paper, it is clear that the cylinder on which the paper is fastened must either remain fixed in darkness, while a cover having an aperture to admit the ray revolves at the same rate as the sun, or else the cylinder must be caused to revolve in the same direction as the sun, but with double the velocity; this last condition, however, would induce needless complication into the wheelwork of the mechanism. The simplest mechanism I have been able to devise, which meets all the conditions, is as follows.

122. To the lower end of the steel rod which penetrates the axis of the hour-index of the watch described in Art. 119, must be firmly adapted a hollow cylinder of thin and light material, as very thin sheet-brass, closed at that end which is attached to the steel rod, but open at the other, and having its axis coincident with the prolongation of the rod. In one side of this cylinder, viz. on that which corresponds to

the hour-angle 0 of the index-hand, must be cut a very narrow longitudinal slit, commencing at the closed end, and extending down somewhat less than two thirds the length of the cylinder, to admit the sunbeam. And it is evident that if this slit be so placed at any moment of the day that the sunbeam admitted through it shall strike the (imaginary) axis of the cylinder, it will do so all day; and that this will take place in the arrangement proposed, if the watch be set to show the correct hour-angle.

123. But in place of freely traversing the hollow of the cylinder, the ray is to be received, immediately on entering the slit, on a band of sensitive paper rolled round an interior *fixed* wooden cylinder, concentric with the other, and barely leaving room for the exterior revolving cylinder to escape contact in every part. This is easily accomplished by forming this interior cylinder of solid wood, bored along its axis, and causing it to travel smoothly along a cylindrical mandril of sufficient strength and solidity of support at its lower end to keep it always well in its true polar direction. For convenience of taking off the old and adapting new papers, this mandril may be furnished with a hinge at its lower end, which, however, must be very strong and of good workmanship; so that being turned back to a proper angle, the wooden cylinder it carries may be enabled to slide entirely off it at its upper end, and in so doing clear the lower rim of the hollow cylinder. Supposing the mandril in this position, the paper fixed round the upper end of the wooden cylinder, the latter replaced on the mandril and thrust down far enough to clear as above mentioned; the mandril must be then restored to its polar direction by turning on its hinge till it abuts against a fixed obstacle, and the wooden cylinder with its paper must then be slided up it till its upper end shall be very near to, but not in contact with the close upper end of the hollow revolving cylinder: and in this situation it is to be pinned or clamped fast. To prevent admission of light between the hollow exterior and wooden interior cylinders, the latter should be formed with a projecting shoulder and metal cylindrical guard-rim, which shall in its turn, when slided up as aforesaid, externally surround the lower part of the hollow cylinder, coming up, all round it, almost to the lower end of the slit which admits the sunbeam. A glance at Plate I. will explain whatever may appear obscure in the description of this mechanism. The others require no illustration. I ought to state that it is the second only of these contrivances that I have actually executed.

Reference to Plate I.

A. The steel axis carried round by the clockwork in twenty-four hours.

B. The hollow brass cylinder open at its lower end to admit the upper portion of the wooden cylinder I.

C. The slit to admit a sunbeam, covered with clear plate-glass.

D. The mandril revolving on the hinge E, and having, when in its working position, its axis coincident with that of the steel rod A and cylinder B.

E. The hinge in east and west azimuth.

F. The obstacle forming part of the bed G H of the apparatus destined to sustain the mandril D at its correct elevation.

G. A similar obstacle to keep it from tilting inconveniently far, when thrown back to take off the wooden cylinder.

H. The bed of the apparatus.

I. The wooden cylinder sliding on the mandril D, and shaped off for lightness at its lower part.

K. The band of sensitive paper rolled round it.

L. An exterior cylindrical guard to cut off light when the cylinder is slided up to its place on the mandril and clamped there, and to prevent the entry of rain below.

M. The weather-tight case, containing a spring clock to drive the axis A.

N. The disc covered with sensitive paper.

O. A close-fitting cover to defend the clockwork from rain, glazed with clear plate-glass at its upper surface P, well cemented. The cylindrical rim acts as a guard to keep the sun's direct rays from striking on the sensitive paper.

Q. A brass diaphragm in the cover O, in which is cut the sector-shaped slit for admission of light, having its direction from the centre of the dial-plate to the hour-line of noon.

R. The pillar supporting the upper works.

Note I.—*On the Distribution of the Calorific Rays in the Solar Spectrum.*

Received March 4,—Read March 5, 1840.

124. Since the communication to the Royal Society, of the foregoing pages, I have discovered a process by which the calorific rays in the solar spectrum are made to leave their impress on a surface properly prepared for the purpose, so as to form what may be called a thermograph of the spectrum, in which the intensity of the thermic ray of any given refrangibility is indicated by the degree of whiteness produced on a black or very dark ground, by the action of the ray at the point where it is received at the surface. From the nature of the process it is demonstrable, that only rays of *heat*, and not those which are usually called chemical, can be concerned in the effect produced.

125. It is easy to see what a variety of interesting applications this lays open to those engaged in studying the subject of transcalescence, whether in its relations to coloured or colourless media. As a specimen of what may be expected from the mode of observation in question, I shall proceed to describe the thermograph of a spectrum transmitted, 1st, through the prism of FRAUNHOFER's flint-glass, described in Art. 57 of the foregoing paper; and 2ndly, through the flint- and crown-lenses of the achromatic object-glass, there also mentioned; premising, however, that the observations were made in occasional and by no means powerful gleams of sun, in a day generally and densely cloudy.

126. Recurring to the dimensions and signs of the former articles, used in describing the chemical and luminous spectra, the thermograph in question consists of a white streak of the breadth of the sun's diameter, extending, where broadest and best defined, towards the positive or more refrangible end of the spectrum, to about + 25 or 30 parts, (of which the old Newtonian spectrum, taking in the extreme visible colours, occupies 54,) but fading away with great rapidity, and at the point so indicated no longer traceable, a point corresponding to about the end of the brighter blue rays. On the negative or less refrangible side of the fiducial yellow, however, it goes on increasing to a maximum at − 16, that is to say, 3·2 parts beyond the extreme red ray. (The dimensions here referred to are reduced to a linear spectrum, or to one formed by a luminary having no sensible diameter.) Beyond this maximum, the intensity of whiteness decreases *to a minimum* at − 31, where it is nearly evanescent, but immediately again recovers, so as to form a faint but unequivocal whitened solar image, *insulated from the rest of the thermic spectrum*, whose centre is situated at − 35; thus imitating, in a certain sense, the coloured spectrum of the cobalt-glass, in which a faint and otherwise indiscernible red ray is insulated from the rest of the coloured spectrum.

127. If any doubt should remain in the minds of photologists as to the correctness of my father's results, which placed the maximum of heat beyond the last visible red,

the ocular evidence of their effects thus afforded must at once set the question at rest. But the extent to which the thermic rays thus traced, overlap the luminous and even the chemical or oxidizing rays, is, I must confess, to myself quite unexpected.

128. If we take in the calorific effect at the least refrangible or negative end, and the chemical at the positive end of the spectrum, the one as traced in the above experiments, the other as in the photographic spectrum of bromuret of silver (Art. 77.), i. e. from − 35 to + 86·9, we have a total of 121·2 for the extent over which dispersion scatters the sun's rays, reckoned on a scale of which the Newtonian coloured spectrum occupies 53·8 parts, or considerably less than one half: and that this is not yet the full extent of prismatic dispersion is highly probable, since the insulation of the terminal thermic image can hardly be considered in any other light than as a result of absorbent action in one or all the glasses through which the rays have passed.

Note II.—*On a definite limit of Photographic Action of the Blue Rays under the influence of an Iodic Salt.*

Received and read with Note I.

129. A spectrum was formed on paper (No. 606.) prepared with alternate washes of nitrate of silver and common salt; and this action was continued till the impression became very intense. *Without dismounting it, or in any way displacing the spectrum from exact coincidence with the photographic image*, it was washed under the continued action of the light, with a moderately dilute solution of hydriodate of potash. The action immediately began to be reversed at the more refrangible end, and as far as the junction of the blue and indigo rays, or rather, to speak more distinctly, as far as + 25·0 in the actual, or + 21·4 in the linear spectrum. After continuing the action a short time, the whole of the more refrangible end of the photograph, up to this limit, was eaten away, the limit being marked by a tolerably sharp and well-defined semicircular outline, while below, the effect was hardly to deteriorate the impression produced. (The actual spectrum was produced to the meeting for inspection).

Note III. *in addition to the foregoing Paper.*

Account of a Process for rendering visible the Calorific Spectrum by its effect on Paper properly prepared, and of some further results obtained respecting the distribution of Heat therein.

Received March 12,—Read March 12, 1840.

130. It is well known to artists in water colours, that their tints, when freshly laid on and wet, are deeper and darker than they ultimately become on drying, a change which must be allowed for in the colouring, or the effect will be spoiled. This is re-

marked in some colours more than in others, and is particularly striking in the case of Indian ink. It depends obviously upon the water wherewith the colours are mixed, rendering, by its refractive power, the paper beneath more penetrable by the incident light, which entering its pores becomes absorbed and lost by numberless partial reflexions.

131. If a paper so over-coloured be dried unequally, those parts which are dry first appear lighter than the rest; and if a heated body be applied at any point behind the paper while wet, the increased evaporation at the points of contact will speedily effect the appearance of a white spot or figure, tracing out the form of the body, or the locus of incident heat where radiant from it; and thus the effects of either conducted or radiant heat may be rendered sensible to the eye. It is of this property, traced a little further into its mode of operation, and exalted by certain processes, which I am about to describe, that advantage may be taken to afford a visible picture of the thermic spectrum.

132. The paper best adapted of any which I have tried for placing in distinct evidence the calorific spectrum, is that called thin post, of the thinnest kind which will bear ink-writing, such as is sold for foreign correspondence. Its thickness, measured by the sphærometer, is 0·00136 inch. One side of this paper is to be blackened with Indian ink, or, which is better, smoked in the flame of oil of turpentine, or over a candle burning with a smoky flame, by drawing it often and quickly through the flame, giving it time to cool between each exposure, till it is coated on the under side with a film of deposited black, as nearly uniform as possible. A slip of the paper so prepared is to be stretched on a frame adjusted to the spectrum-apparatus described in the foregoing pages, so as to present its white side to the incident spectrum, keeping the blackened side hollow to admit air, and to avoid rubbing off the black coat*. A fiducial dot being made on it, and brought to coincidence with the standard yellow ray of the spectrum, a flat brush, equal in breadth to the paper, dipped in good rectified spirit of wine, is to be passed over the white surface till the paper is completely saturated, which will be indicated by its acquiring a uniform blackness in place of the white it at first exhibited. After a few moments' exposure, a whitish spot begins to appear considerably below the extreme red end of the luminous spectrum, (supposing the violet end uppermost,) which increases rapidly in breadth until it equals the breadth of the luminous spectrum, and even somewhat surpasses it; and in length, till it forms a long appendage exterior to the spectrum, and extends moreover within it up to and beyond the fiducial yellow. In this state, and just as the general drying of the paper begins, by whitening the whole surface, to confuse the appearances, a second, sudden, and copious wash of alcohol from above downwards must be applied without disturbing the spectrum, or in any way shaking the apparatus. The super-

* The effect is more marked if the black side be turned to the incident rays, and the white side be viewed from behind, by using an open frame. But in this mode of observing, a strong light must be thrown on the white side, to enable the spectator to see and measure the effect; and at the same moment the spectrum must be intercepted, which is inconvenient unless an assistant be present.

fluous alcohol will have hardly run off, when the phenomena of the thermic spectrum begin to appear in all their characters, at first faintly, and, as it were, sketched in by a dimness and dullness of the otherwise shining and reflective surface of the wetted paper; but this is speedily exchanged for a perfect whiteness, marking, by a clear and sharp outline, the lateral extent of the calorific rays, and by due gradations of intensity, in a longitudinal direction, their law or scale of distribution, both within and without the luminous spectrum.

133. It may appear at first sight as if the process above described for the preparation of the paper were not in exact accordance with the principle of action at first explained. But a little consideration will show, that, as a coloured paper dries, it is the *pile* or projecting filaments which the film of moisture retreating into the paper lays bare, which first catch and reflect the light. By transferring the colour to the *under* side of thin paper, we in effect only provide for the regular and copious exhibition of such reflective filaments, unsullied by the application of colouring matter. The rationale of this will be obvious on trying a few experiments on papers coloured through their whole substance, of which the best tints are blue, and that kind of greenish brown which is in very common use among bookbinders; and the best texture a soft and bibulous one, into which liquids readily sink, and form very dark spots while wet.

134. The kind of paper is by no means indifferent in another and very important respect. It is with thermographic as with photographic papers; each has its peculiar *scale of action*. Each absorbs in preference some above others of the thermic rays, and thereby gives an undue preponderance of effect to those so preferred. It is therefore only by trials on a great variety of papers that we can satisfy ourselves whether any observed interruptions or deficiencies in the thermic spectrum be really due to the absence of calorific rays of corresponding refrangibility in the incident beam, or be not simply owing to partial insensibility in the particular paper used. The smoked paper I have above recommended seems to be more free than any other I have tried from this defect. That prepared with Indian ink is materially less sensitive, both to the most and least refracted heat-rays. In this respect, the smoked compared with the inked paper holds the same sort of relation as the bromuretted paper compared with a merely nitrated one, in respect of the chemical action.

135. Supposing, then, (unless where the contrary is expressed,) that such smoked paper is employed, the thermograph of the spectrum in its most complete state, or rather in that succession of states in which each part comes most characteristically into view, is as represented in Plate II. fig. 2, where the dimensions are enlarged in the ratio of three to two above those actually measured. On the same enlarged scale, fig. 3. represents the luminous spectrum as seen with the naked eye, and fig. 4. as seen through the standard cobalt-glass used in the above-recited experiments; while fig. 5. (still on the same scale) represents the photographic spectrum impressed on bromuretted paper*; and

* See Art. 77.

(in order to show the greatest extent to which I have observed the whitening or conservative action of the less refrangible chemical rays to extend, and that only in one and a very peculiar case,) fig. 6. exhibits a spectrum photographically impressed on paper (No. 610.) originally prepared with successive washes of nitrate of lead, muriate of soda, and nitrate of silver, but kept for some time (five months), and thereby much impaired in point of sensibility. The same paper fresh prepared exhibits no such anomalous spectrum.

136. The most singular and striking phenomenon exhibited in the thermic spectrum thus visibly impressed, is its want of continuity. It obviously consists of several distinct patches, of which α, β are the most conspicuous and intense, but are less distinctly separated, and of which when the sun is very strong and clear it is even difficult to trace the separation. The spot γ, on the other hand, is round and well insulated. It begins to appear on the paper soon after the ovals α, β are fully formed, and when β has assumed a sharply rounded outline. The first symptom of its appearance is the dulling of the wet and shining surface of the paper, which is speedily followed by the appearance of a small round white speck; this continues to increase rapidly in size and whiteness, and at length assumes a definite and perfectly circular outline, within which the paper is entirely white. By degrees the oval β and this spot join and run together, forming a white streak deeply indented at the point of junction. It is not till this happens that similar symptoms begin to betray the existence of a still more remote heat-spot δ. Indeed, it generally requires another wash of alcohol before this can be fully brought into evidence. It is, however, perfectly unequivocal, though very much feebler, and rather worse defined than γ, with which also it is somewhat better connected than γ with β.

137. The mean of a great number of measures, as precise as the nature of the observation admits, gives for the situation of the centres or maxima of these spots, or their coordinates along the axis of the spectrum, reckoning as usual from the fiducial yellow, as follows:

α — 17·5, 19·0, 18·5, 16·0, 19, 19, 20, 18, 19, 20, 20, 19, 16, 16, 16, 18·5, 18·3. Mean of all = — 18·2.

β — 26·0, 27·5, 29·0, 25·0, 25·5, 27·3. Mean — 26·7.

γ — 36·0, 35, 35, 34·5, 36, 36, 35·5, 36, 36, 36, 36, 36·5, 37·2, 35·5, 36·5, 36·0, 35·0, 36·2, 36·0, 35·5, 36·5. Mean — 35·7.

δ — 45, 46, 46·5, 45·0, 44·2, 44·3, 44·5, 46, 44, 45·8, 45·5. Mean — 45·1.

The measures for α and β differ a good deal *inter se*, partly from the less definite nature of the maxima, and partly from having used in the measures a variety of different sorts of paper, which influence the extent and intensity of the spots α and β materially. The measures of γ and δ are closer by reason of the definite nature of the rays which respectively go to form them. Nor does the nature of the paper used in any respect displace these two spots, though it influences to a great extent their apparent

intensity, and in some cases obliterates them. Indeed some coloured papers refuse to exhibit any thermographic impress whatever.

138. Of the existence of a much more remote spot I have hardly a doubt; but it is very difficult to obtain a sight of it. Its place is at or about — 55, as measured on the first occasion, when it appeared with sufficient distinctness to be set down as a positive observation, at the same time that the spot δ was placed by a similar measure at — 45·2. On another occasion (March 8), the sun being superbly clear, four measures, considered at the time quite satisfactory, were obtained of it, the mean of which was — 63·1; but this requires a correction for the position of the prism, which on this occasion was somewhat different from that which it had in previous experiments, it having been adjusted so as to throw the spectrum higher on the frame than before, for the sake of allowing plenty of room for the exhibition of the phænomena in question. But as on this occasion very numerous measures were also taken of the other spots, there are fortunately abundant data for calculating and applying the correction in question. I therefore set down all the measures so taken in the following Table.

Coord. of spot	α	β	γ	δ	ϵ
	— 23·0	— 28·0	— 38·5	— 48·4	— 62·7
	21·5	31·5	41·0	46·4	62·7
	19·0	31·8	41·5	49·0	63·0
	19·0	29·0	38·3	49·5	64·0
		29·5	39·5	50·0	
		30·2	40·0	50·4	
		33·0	41·5	50·5	
			41·0	49·0	
			40·0	48·8	
				50·0	
				51·0	
				51·0	
				50·2	
				50·0	
				52·0	
Means	— 20·6	— 30·4	— 40·2	— 49·7	— 63·1

If these numbers be now all reduced in the proportion of 40·2 to 35·7, so as to bring the spot γ, which is the best-defined of any, to correspondence with the former results, we have for the successive coordinates as follows:

α	β	γ	δ	ϵ
— 18·2	— 27·0	— 35·7	— 44·1	— 56·0

139. Whether these measures be considered or not as definitively establishing the existence of the spot ϵ, the equality of distribution of them all along the axis of the spectrum is not a little remarkable, and would at first sight (from the analogy of the absorptive action of nitrous gas on the luminous rays) seem to point rather to a

gaseous than a vitreous absorption as their origin. As I have no reason for preferring one to the other set of results, I shall take a mean of the two sets, which, with the differences of the coordinates or intervals between the centres of the spots, run as follows:

	Coordinates.	Differences.
Spot α....	− 18·20	
β....	− 26·85	8·65
γ....	− 35·70	8·65 } Mean 8·80
δ....	− 44·60	8·90
ϵ....	− 55·50	10·90

If we now consider that the half of 18·2 is 9·1, we might almost be led to expect a subdivision of the spot α, with its train*, into others, one of which should have its maximum at the fiducial point Y, and another half-way between that point and α. And though I am not disposed to attribute much weight to the fact, it is right to mention that when paper prepared with Indian ink was used, and a very pale yellow glass interposed between the lens and the spectrum, an oval spot, having its centre precisely on the point Y, *was* actually insulated. And this experiment, it should also in justice be mentioned, was made prior to the assemblage and reduction of the above measures, and in the absence of any speculative views concerning them.

140. The gaseous media through which the rays have reached their point of action, are the atmospheres of the sun and earth. The effect of the former is beyond our control, unless we could carry our experiments to such a point of delicacy as to operate separately on rays emanating from the centre and borders of the sun's disc. That of the earth's, though it cannot be eliminated any more than in the case of the sun's, may yet be varied to a considerable extent by experiments made at great elevations and under a vertical sun, and compared with others where the sun is more oblique, the situation lower, and the atmospheric pressure of a temporarily high amount. Should it be found that this cause is in reality concerned in the production of the spots, we should see reason to believe that a large portion of solar heat never reaches the earth's surface, and that what is incident on the summits of lofty mountains differs not only in quantity, but also in *quality*, from what the plains receive.

141. The solid media traversed by the rays in our experiments above related, are crown- and flint-glass, the latter of two varieties, though most probably differing little in composition, viz. that of which the prism and the concave lens of the object-glass are formed. On substituting a crown- for the flint-prism, the spot γ was insulated as before, but α and β were less evidently separated. When the achromatic lens was suppressed, and (still using the crown-prism) a very thin double convex crown-lens, of nearly equal focal length, was used to collect the rays, so as to get rid of flint-glass altogether, the insulation of γ was much less sensible, and the separation of α and β hardly to be perceived. This would go to point out the flint-glass as the origin of

* See Plate II. fig. 2.

the spots, and to that idea I rather incline. But the spectrum is so much contracted in length when a crown-prism is used, that it is not easy to say with certainty how far the apparent obliteration of the separating minima might be attributable to that cause.

142. When the spectrum formed by the flint-prism and achromatic lens, as usual, was displaced laterally by refraction through the thick part of a crown-prism, but little alteration took place in the relative intensities of α, β, and γ, which experiment, so far as it goes, corroborates the conclusion of the last paragraph.

143. A very different result was obtained by substituting a water-prism. The spot γ was greatly enfeebled by this, nor could δ be traced at all. The scale of transcalescence of water, therefore, would appear to be considerably more contracted than that of either flint- or crown-glass. And the same conclusion was arrived at by interposing between the lens and its focus a parallel-sided box of plate-glass. It having been first ascertained that the box when empty allowed all the spots, α, β, γ, δ, to pass freely, it was filled with distilled water, which completely destroyed both γ and δ; and so far curtailed β, that it could no longer be recognised as distinct, the thermic action being terminated at -24 by a sharp and well-defined oval outline. The upper portion of the thermic spectrum, extending above the yellow into the green and blue rays, was not attacked. That the spot γ was produced when a water *prism* was used, must be attributed to the thinness of that medium at the edge. When the box was used all the rays had to pass through 1·4 inch of water, that being the interior interval between the transmitting surfaces.

144. When muriate of lime (a saturated solution) was substituted for water, the result was nearly as with pure water, the action being cut off at -26, but not quite so sharply. I ought also to notice that it was in this experiment that the first suspicion of the existence of ϵ was suggested, by which it might seem that the absorptive action of this medium relaxes for the extreme thermic rays. Nitrate of silver (specific gravity 1·200) and nitrate of lead gave the same results as water. The sides of my glass box being united with a resinous cement, I could not try the effect of alcohol, ether, essential oils, &c.; nor has time permitted any further extension of the inquiry in this direction. Colourless quartz transmits α, β, and γ freely.

145. Green glasses cut off nearly the whole thermic spectrum. The oval outline of β, which passes between β and γ, as also the spot γ itself, may, however, be traced. A standard red combination, which insulated the extreme red by the joint action of a cobalt-blue and a dark brown glass, cuts off both γ and δ, and reduces the thermic spectrum to two white spots, α and β, of which the latter is greatly enfeebled. The upper edge of the spot α (which is greatly curtailed in both directions) somewhat overlaps the lower edge of the red image of the sun formed by this glass. All the more refrangible thermic rays are cut off by this combination, as they are by a cobalt-glass used singly, which latter, however, allows γ to pass freely enough.

146. A brown glass of moderate intensity transmitted α, β, and γ, the latter copi-

ously, and β more so than α; its scale of transcalescence is therefore in analogy with its colour. It has been already mentioned that a pale yellow glass insulated a spot coincident with the fiducial point Y. A solution of muriate of chromium transmitted α and β, the former most copiously, but stopped the rest.

147. In the mode of observation above described the thermic spectrum is rendered only transiently visible; ample time, indeed, is afforded for all needful measurements, and for any examination, however minute; but it seemed desirable to obtain a permanent impression or a *fixed thermograph*, capable of being preserved as a record. In this I have been to a certain extent successful, and I doubt not shall very speedily be enabled to accomplish completely this desirable object. The method which I have hitherto found to succeed best, is to dissolve in the alcohol used for washing the paper a small portion of some colouring matter, which being deposited in the pores of the paper at those points where the evaporation goes on most rapidly, in greater quantity than where it proceeds slowly, becomes accumulated on those points by successive washes to such an amount as to indicate by a marked difference of colour the distribution of the calorific rays. The colouring matter of the *Viola tricolor* is very well adapted for this purpose, as it does not develop its colour from the alcoholic tincture immediately, but requires time for oxidation, so that the experiment is not interfered with by the paper being discoloured while exposed to the spectrum.

148. To exhibit the polarization of solar heat, the following method was employed. In place of the prism, a plate of glass inclined at the polarizing angle to the axis of the lens was substituted, and intermediate between the lens and its focus a glass plate, also inclined to its axis at that angle, but in a plane at right angles to that of the first reflexion, was adapted. The focus, after reflexion at this plate, was received on prepared paper, when it formed a faint image, which of course would have been invisible were all the rays *completely* polarized. As it was, it proved too feeble to act thermographically on the paper when wetted with alcohol, thereby showing that no appreciable quantity of heat had undergone reflexion; in other words, that the thermic as well as luminous rays had received the polarized character. A plate of mica was now interposed between the lens and the first glass-plate, so inclined as to polarize at an angle of 45° a considerable portion of the reflected beam, and thereby render it susceptible of reflexion at the second glass-plate. And it was now found that the focal image, which in this arrangement was considerably bright, had acquired the power of imprinting itself thermographically on the paper, and leaving there a well-defined circular white spot. The same effect was obtained when an unannealed glass disc was used to disturb the polarity of the cone of rays.

J. F. W. H.

II. *Experimental Researches in Electricity.—Sixteenth Series.*

By Michael Faraday, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, &c. &c.*

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§ 24. *On the source of power in the voltaic pile.*

¶ i. *Exciting electrolytes, &c. being conductors of thermo and feeble currents.*
¶ ii. *Inactive conducting circles containing an electrolytic fluid.*
¶ iii. *Active circles excited by solution of sulphuret of potassium, &c.*

§ 24. *On the source of power in the voltaic pile.*

1796. WHAT is the source of power in a voltaic pile? This question is at present of the utmost importance in the theory and to the development of electrical science. The opinions held respecting it are various; but by far the most important are the two which respectively find the source of power in contact, and in chemical force. The question between them touches the first principles of electrical action; for the opinions are in such contrast, that two men respectively adopting them are thenceforward constrained to differ, in every point, respecting the probable and intimate nature of the agent or force on which all the phenomena of the voltaic pile depend.

1797. The theory of contact is the theory of Volta, the great discoverer of the voltaic pile itself, and it has been sustained since his day by a host of philosophers, amongst whom, in recent times, rank such men as Pfaff, Marianini, Fechner, Zamboni, Matteucci, Karsten, Bouchardat, and as to the excitement of the power, even Davy; all bright stars in the exalted regions of science. The theory of chemical action was first advanced by Fabroni*, Wollaston†, and Parrot‡, and has been more or less developed since by Œrsted, Becquerel, De la Rive, Ritchie, Pouillet, Schœnbein, and many others, amongst whom Becquerel ought to be distinguished as having contributed, from the first, a continually increasing mass of the strongest

* A.D. 1792, 1799. Becquerel's Traité de l'Électricité, i. pp. 81—91, and Nicholson's Quarto Journal, iii. 308. iv. 120, or Journal de Physique, vi. 348.

† A.D. 1801. Philosophical Transactions, 1801, p. 427.

‡ A.D. 1801. Annales de Chimie, 1829, xliii. 45; 1831, xlvi. 361.

experimental evidence in proof that chemical action always evolves electricity*; and DE LA RIVE should be named as most clear and constant in his views, and most zealous in his production of facts and arguments, from the year 1827 to the present time†.

1798. Examining this question by the results of definite electro-chemical action, I felt constrained to take part with those who believed the origin of voltaic power to consist in chemical action alone (875. 965.), and ventured a paper on it in April 1834‡ (875, &c.), which obtained the especial notice of MARIANINI§. The rank of this philosopher, the observation of FECHNER‖, and the consciousness that over the greater part of Italy and Germany the contact theory still prevailed, have induced me to re-examine the question most carefully. I wished not merely to escape from error, but was anxious to convince myself of the truth of the contact theory; for it was evident that if contact electromotive force had any existence, it must be a power not merely unlike every other natural power as to the phenomena it could produce, but also in the far higher points of limitation, definite force, and finite production (2065.).

1799. I venture to hope that the experimental results and arguments which have been thus gathered may be useful to science. I fear the detail will be tedious, but that is a necessary consequence of the state of the subject. The contact theory has long had possession of men's minds, is sustained by a great weight of authority, and for years had almost undisputed sway in some parts of Europe. If it be an error, it can only be rooted out by a great amount of forcible experimental evidence; a fact sufficiently clear to my mind by the circumstance, that DE LA RIVE's papers have not already convinced the workers upon this subject. Hence the reason why I have thought it needful to add my further testimony to his and that of others, entering into detail and multiplying facts in a proportion far beyond any which would have been required for the proof and promulgation of a new scientific truth (2017.). In so doing, I may occasionally be only enlarging, yet then I hope strengthening, what others, and especially DE LA RIVE, have done.

1800. It will tend to clear the question, if the various views of contact are first stated. VOLTA's theory is, that the simple contact of conducting bodies causes electricity to be developed at the point of contact without any change in nature of the bodies themselves; and that though such conductors as water and aqueous fluids

* A.D. 1824, &c. Annales de Chimie, 1824, xxv. 405; 1827, xxxv. 113; 1831, xlvi. 265, 276, 337, xlvii. 113, xlix. 131.

† Ibid. 1828, xxxvii. 225, xxxix. 297; 1836, lxii. 147: or Mémoires de Genève, 1829, iv. 285; 1832, vi. 149; 1835, vii.

‡ Philosophical Transactions, 1834, p. 425.

§ Memorie della Società Italiana in Modena, 1837, xxi. p. 205.

‖ Philosophical Magazine, 1838, xiii. 205; or POGGENDORF's Annalen, xlii. p. 481. FECHNER refers also to PFAFF's reply to my paper. I never cease to regret that the German is a sealed language to me.

have this property, yet the degree in which they possess it is unworthy of consideration in comparison with the degree to which it rises amongst the metals*. The present views of the Italian and German contact philosophers are, I believe, generally the same, except that occasionally more importance is attached to the contact of the imperfect conductors with the metals. Thus Zamboni (in 1837) considers the metallic contact as the most powerful source of electricity, and not that of the metals with the fluids†; but Karsten, holding the contact theory, transfers the electromotive force to the contact of the fluids with the solid conductors‡. Marianini holds the same view of the principle of contact, with this addition, that actual contact is not required to the exertion of the exciting force, but that the two approximated dissimilar conductors may affect each other's state, when separated by sensible intervals of the $\frac{1}{10000}$dth of a line and more, air intervening§.

1801. De la Rive, on the contrary, contends for simple and strict chemical action, and, as far as I am aware, admits of no current in the voltaic pile that is not conjoined with and dependent upon a complete chemical effect. That admirable electrician Becquerel, though expressing himself with great caution, seems to admit the possibility of chemical attractions being able to produce electrical currents when they are not strong enough to overcome the force of cohesion, and so terminate in combination‖. Schœnbein states that a current may be produced by a tendency to chemical action, i. e. that substances which have a tendency to unite chemically may produce a current, though that tendency is not followed up by the actual combination of the substances¶. In these cases the assigned force becomes the same as the contact of Volta, inasmuch as the acting matters are not altered whilst producing the current. Davy's opinion was, that contact like that of Volta excited the current or was the cause of it, but that chemical changes supplied the current. For myself I am at present of the opinion which De la Rive holds, and do not think that, in the voltaic pile, mere contact does anything in the excitation of the current, except as it is preparatory to, and ends in, complete chemical action (1741. 1745.).

1802. Thus the views of contact vary, and it may be said that they pass gradually from one to another, even to the extent of including chemical action: but the two extremes appear to me irreconcilable in principle under any shape; they are as follows. The contact theory assumes, that when two different bodies being conductors of electricity are in contact, there is a force at the point of contact by which one of the bodies gives a part of its natural portion of electricity to the other body, which the latter takes in addition to its own natural portion; that, though the touching points have thus respectively given and taken electricity, they cannot retain the charge which their contact has caused, but discharge their electricities to the masses

* Annales de Chimie, 1802, xl. p. 225.
† Bibliothèque Universelle, 1836, v. 387; 1837, viii. 189.
‡ L'Institute, No. 150.
§ Mem. della Soc. Ital. in Modena, 1837, xxi. 232–237.
‖ Annales de Chimie, 1835, lx. 171; and Traité de l'Électricité, i. pp. 253, 258.
¶ Philosophical Magazine, 1838, xii. 227, 311, 314; also Bibliothèque Universelle, 1838, xiv. 155, 395.

respectively behind them (2067.): that the force which, at the point of contact, induces the particles to assume a new state, cannot enable them to keep that state (2069.): that all this happens without any permanent alteration of the parts that are in contact, and has no reference to their chemical forces (2065. 2069.).

1803. The chemical theory assumes, that at the place of action, the particles which are in contact act chemically upon each other and are able, under the circumstances, to throw more or less of the acting force into a dynamic form (947. 996. 1120.): that in the most favourable circumstances, the whole is converted into dynamic force (1000.): that then the amount of current force produced is an exact equivalent of the original chemical force employed; and that in no case (in the voltaic pile) can any electric current be produced, without the active exertion and consumption of an equal amount of chemical force, ending in a given amount of chemical change.

1804. MARIANINI's paper* was to me a great motive for re-examining the subject; but the course I have taken was not so much for the purpose of answering particular objections, as for the procuring evidence, whether relating to controverted points or not, which should be satisfactory to my own mind, open to receive either one theory or the other. This paper, therefore, is not controversial, but contains further facts and proofs of the truth of DE LA RIVE's views. The cases MARIANINI puts are of extreme interest, and all his objections must, one day, be answered, when numerical results, both as to intensity and quantity of force, are obtained; but they are all debateable, and, to my mind, depend upon variations of quantity which do not affect seriously the general question. Thus, when that philosopher quotes the numerical results obtained by considering two metals with fluids at their opposite extremities which tend to form counter currents, the difference which he puts down to the effect of metallic contact, either made or interrupted, I think accountable for, on the facts partly known respecting opposed currents; and with me differences quite as great, and greater, have arisen, and are given in former papers (1046.), when metallic contacts were in the circuit. So at page 213 of his memoir, I cannot admit that *e* should give an effect equal to the difference of *b* and *d*; for in *b* and *d* the opposition presented to the excited currents is merely that of a bad conductor, but in the case of *e* the opposition arises from the power of an opposed acting source of a current.

1805. As to the part of his memoir respecting the action of sulphuretted solutions†, I hope to be allowed to refer to the investigations made further on. I do not find, as the Italian philosopher, that iron with gold or platina, in solution of the sulphuret of potassa, is positive to them‡, but, on the contrary, powerfully negative, and for reasons given in the sequel (2049.).

1806. With respect to the discussion of the cause of the spark before contact§,

* Memorie della Società Italiana in Modena, 1827, xxi. p. 205.
† Ibid. p. 217.
‡ Ibid. p. 217.
§ Ibid. p. 225.

Marianini admits the spark, but I give it up altogether. Jacobi's paper* convinces me I was in error as to *that proof* of the existence of a state of tension in the metals before contact (915. 956.). I need not therefore do more at present than withdraw my own observations.

1807. I now proceed to address myself to the general argument, rather than to particular controversy, or to the discussion of cases feeble in power and doubtful in nature; for I have been impressed from the first with the feeling that it is no weak influence or feeble phenomenon that we have to account for, but such as indicates a force of extreme power, requiring, therefore, that the cause assigned should bear some proportion, both in intensity and quantity, to the effects produced.

1808. The investigations have all been made by aid of currents and the galvanometer, for it seemed that such an instrument and such a course were best suited to an examination of the electricity of the voltaic pile. The electrometer is no doubt a most important instrument, but the philosophers who do use it are not of accord in respect to the safety and delicacy of its results. And even if the few indications as yet given by the electrometer be accepted as correct, they are far too general to settle the question of, whether contact or chemical action is the exciting force in the voltaic battery. To apply that instrument closely and render it of any force in supplying affirmative arguments to either theory, it would be necessary to construct a table of contacts, or the effects of contacts, of the different metals and fluids concerned in the construction of the voltaic pile, taken in pairs (1868.), expressing in such table both the *direction* and the *amount* of the contact force.

1809. It is assumed by the supporters of the contact theory, that though the metals exert strong electromotive forces at their points of contact with each other, yet these are so balanced in a metallic circuit that no current is ever produced whatever their arrangement may be. So in Plate III. fig. 1. if the contact force of copper and zinc is 10 ⟶, and a third metal be introduced at *m*, the effect of its contacts, whatever that metal may be, with the zinc and copper at *b* and *c*, will be an amount of force in the opposite direction = ⟵ 10. Thus, if it were potassium, its contact force at *b* might be 5 ⟶, but then its contact force at *c* would be ⟵ 15: or if it were gold, its contact force at *b* might be ⟵ 19, but then its contact force at *c* would be 9 ⟶. This is a very large assumption, and that the theory may agree with the facts is necessary: still it is, I believe, only an assumption, for I am not aware of any data, independent of the theory in question, which prove its truth.

1810. On the other hand, it is assumed that fluid conductors, and such bodies as contain water, or, in a word, those which I have called electrolytes (664. 823. 921.), either exert no contact force at their place of contact with the metals, or if they do exert such a power, then it is with this most important difference, that the forces are not subject to the same law of compensation or neutralization in the complete circuit, as holds with the metals (1809.). But this, I think I am justified in saying, is an as-

* Philosophical Magazine, 1838, xiii. 401.

sumption also, for it is supported not by any independent measurement or facts (1808.), but only by the theory which it is itself intended to support.

1811. Guided by this opinion, and with a view to ascertain what is, in an active circle, effected by contact and what by chemical action, I endeavoured to find some bodies in this latter class (1810.) which should be without chemical action on the metals employed, so as to exclude that cause of a current, and yet such good conductors of electricity as to show any currents due to the contact of these metals with each other or with the fluid: concluding that any electrolyte which would conduct the thermo current of a single pair of bismuth and antimony plates would serve the required purpose, I sought for such, and fortunately soon found them.

¶ i. *Exciting electrolytes, &c., being conductors of thermo and feeble currents.*

1812. *Sulphuret of potassium.*—This substance and its solution were prepared as follows. Equal weights of caustic potash (potassa fusa) and sulphur were mixed and heated gradually in a Florence flask, till the whole had fuzed and united, and the sulphur in excess began to sublime. It was then cooled and dissolved in water, so as to form a strong solution, which by standing became quite clear.

1813. A portion of this solution was included in a circuit containing a galvanometer and a pair of antimony and bismuth plates; the connexion with the electrolyte was made by two platinum plates, each about two inches long and half an inch wide: nearly the whole of each was immersed, and they were about half an inch apart. When the circuit was completed, and all at the same temperature, there was no current; but the moment the junction of the antimony and bismuth was either heated or cooled, the corresponding thermo current was produced, causing the galvanometer-needle to be permanently deflected, occasionally as much as 80°. Even the small difference of temperature occasioned by touching the SEEBECK element with the finger, produced a very sensible current through the electrolyte. When in place of the antimony-bismuth combination mere wires of *copper and platinum*, or *iron and platinum* were used, the application of the spirit-lamp to the junction of these metals produced a thermo current which instantly travelled round the circuit.

1814. Thus this electrolyte will, as to high conducting power, fully answer the condition required (1811.). It is so excellent in this respect, that I was able to send the thermo current of a single SEEBECK's element across five successive portions connected with each other by platinum plates.

1815. *Nitrous acid.*—Yellow anhydrous nitrous acid, made by distilling dry nitrate of lead, being put into a glass tube and included in a circuit with the antimony-bismuth arrangement and the galvanometer, gave no indication of the passage of the thermo current, though the immersed electrodes consisted each of about four inches in length of moderately thick platinum wire, and were not above a quarter of an inch apart.

1816. A portion of this acid was mixed with nearly its volume of pure water; the

resulting action caused depression of temperature, the evolution of some nitrous gas, the formation of some nitric acid, and a dark green fluid was produced. This was now such an excellent conductor of electricity, that almost the feeblest current could pass it. That produced by SEEBECK's circle was sensible when only one eighth of an inch in length of the platinum wires dipped in the acid. When a couple of inches of each electrode was in the fluid, the conduction was so good, that it made very little difference at the galvanometer whether the platinum wires touched each other in the fluid, or were a quarter of an inch apart*.

1817. *Nitric acid.*—Some pure nitric acid was boiled to drive off all the nitrous acid, and then cooled. Being included in the circuit by platinum plates (1813.), it was found to conduct so badly that the effect of the antimony-bismuth pair, when the difference of temperature was at the greatest, was scarcely perceptible at the galvanometer.

1818. On using a pale yellow acid, otherwise pure, it was found to possess rather more conducting power than the former. On employing a red nitric acid, it was found to conduct the thermo-current very well. On adding some of the green nitrous acid (1816.) to the colourless nitric acid, the mixture acquired high conducting powers. Hence it is evident that nitric acid is not a good conductor when pure, but that the presence of nitrous acid in it (conjointly probably with water), gives it this power in a very high degree amongst electrolytes†. A very red strong nitric acid, and a weak green acid, (consisting of one vol. strong nitric acid and two vols. of water, which had been rendered green by the action of the negative platinum electrode of a voltaic battery,) were both such excellent conductors that the thermo current could pass across five separate portions of them connected by platinum plates, with so little retardation that I believe twenty interruptions would not have stopped this feeble current.

1819. *Sulphuric acid.*—Strong oil of vitriol, when between platinum electrodes (1813.), conducted the antimony-bismuth thermo current sensibly, but feebly. A mixture of two volumes acid and one volume water conducted much better, but not nearly so well as the two former electrolytes (1814. 1816.). A mixture of one volume of oil of vitriol and two volumes saturated solution of sulphate of copper conducted this feeble current very fairly.

Potassa.—A strong solution of caustic potassa, between platinum plates, conducted the thermo current sensibly, but very feebly.

1820. I will take the liberty of describing here, as the most convenient place, other

* DE LA RIVE has pointed out the facility with which an electric current passes between platinum and nitrous acid. Annales de Chimie, 1828, xxxvii. 278.

† SCHOENBEIN's experiments on a compound of nitric and nitrous acids will probably bear upon and illustrate this subject. Bibliothèque Universelle, 1817, x. 406.

results relating to the conducting power of bodies, which will be required hereafter in these investigations. Galena, yellow sulphuret of iron, arsenical pyrites, native sulphuret of copper and iron, native gray artificial sulphuret of copper, sulphurets of bismuth, iron, and copper, globules of oxide of burnt iron, oxide of iron by heat or scale oxide, conducted the thermo current very well. Native peroxide of manganese and peroxide of lead conducted it moderately well.

1821. The following are bodies, in some respect analogous in nature and composition, which did not sensibly conduct this weak current when the contact surfaces were small. Artificial gray sulphuret of tin, blende, cinnabar, hæmatite, Elba iron-ore, native magnetic oxide of iron, native peroxide of tin or tinstone, wolfram, fuzed and cooled protoxide of copper, peroxide of mercury.

1822. Some of the foregoing substances are very remarkable in their conducting power. This is the case with the solution of sulphuret of potassium (1813.) and the nitrous acid (1816.), for the great amount of this power. The peroxide of manganese and lead are still more remarkable for possessing this power, because the *protoxides* of these metals do not conduct either the feeble thermo current or a far more powerful one from a voltaic battery. This circumstance made me especially anxious to verify the point with the peroxide of lead. I therefore prepared some from red-lead by the action of successive portions of nitric acid, then boiled the brown oxide, so obtained, in several portions of distilled water, for days together, until every trace of nitric acid and nitrate of lead had been removed; after which it was well and perfectly dried. Still, when a heap of it in powder, and consequently in very imperfect contact throughout its own mass, was pressed between two plates of platinum and so brought into the thermo-electric circuit (1813.), the current was found to pass readily.

¶ ii. *Inactive conducting circles containing a fluid or electrolyte.*

1823. De la Rive has already quoted the case of potash, iron and platina*, to show that where there was no chemical action there was no current. My object is to increase the number of such cases; to use other fluids than potash, and such as have good conducting power for weak currents; to use also strong and weak solutions; and thus to accumulate the conjoint experimental and argumentative evidence by which the great question must finally be decided.

1824. I first used the sulphuret of potassium as an electrolyte of good conducting power, but chemically inactive (1811.) when associated with iron and platinum in a circuit. The arrangement is given in fig. 2, where D, E represent two test-glasses containing the strong solution of sulphuret of potassium (1812.); and also four metallic plates, about 0·5 of an inch wide and two inches long in the immersed part, of which the three marked P, P, P were platinum, and that marked I, of clean iron: these were connected by iron and platinum wires, as in fig. 2., a galvanometer being

* Philosophical Magazine, 1837, xi. 275.

introduced at G. In this arrangement there were three metallic contacts of platinum and iron, *a b* and *x*: the two first being opposed to each other, may be considered as neutralizing each other's forces; but the third, being unopposed by any other metallic contact, can be compared with either the difference of *a* and *b* when one is warmer than the other, or with itself when in a heated or cooled state (1830.), or with the force of chemical action when any body capable of such action is introduced there (1831.).

1825. When this arrangement is completed and in order, there is absolutely no current circulating through it, and the galvanometer-needle rests at 0°; yet is the whole circuit open to a very feeble current, for a difference of temperature at any one of the junctions *a*, *b*, or *x*, causes a corresponding thermo current, which is instantly detected by the galvanometer, the needle standing permanently at 30° or 40°, or even 50°.

1826. But to obtain this proper and normal state, it is necessary that certain precautions be attended to. In the first place, if the circuit be complete in every part except for the immersion of the iron and platinum plates into the cup D, then, upon their introduction, a current will be produced directed from the platinum (which appears to be positive) through the solution to the iron; this will continue perhaps five or ten minutes, or if the iron has been carelessly cleaned, for several hours; it is due to an action of the sulphuretted solution on *oxide of iron*, and not to any effect on the metallic iron; and when it has ceased, the disturbing cause may be considered as exhausted. The experimental proofs of the truth of this explanation, I will quote hereafter (2049.).

1827. Another precaution relates to the effect of accidental movements of the plates in the solution. If two platinum plates be put into a solution of this sulphuret of potassium, and the circuit be then completed, including a galvanometer, the arrangement, if perfect, will show no current; but if one of the plates be lifted up into the air for a few seconds and then replaced, it will be negative to the other, and produce a current lasting for a short time*. If the two plates be iron and platinum, or of any other metal or substance not acted on by the sulphuret, the same effect will be produced. In these cases, the current is due to the change wrought by the air on the film of sulphuretted solution adhering to the removed plate†; but a far less cause than this will produce a current, for if one of the platinum plates be removed, washed well, dried, and even heated, it will, on its reintroduction, almost certainly exhibit the negative state for a second or two.

1828. These or other disturbing causes appear the greater in these experiments in

* Marianini observed effects of this kind produced by exposure to the air, of one of two plates dipped in nitric acid. Annales de Chimie, 1830, xlv. p. 42.

† Becquerel long since referred to the effect of such exposure of a plate, dipped in certain solutions, to the air. Generally the plate so exposed became positive on re-immersion. Annales de Chimie, 1824, xxv. 405.

consequence of the excellent conducting power of the solution used; but they do not occur if care be taken to avoid any disturbance of the plates or the solution, and then, as before said, the whole acquires a normal and perfectly inactive state.

1829. Here then is an arrangement in which the contact of platinum and iron at x is at liberty to produce any effect which such a contact may have the power of producing; and yet what is the consequence? absolutely nothing. This is not because the electrolyte is so bad a conductor that a current of contact cannot pass, for currents far feebler than this is assumed to be, pass readily (1813.); and the electrolyte employed is vastly superior in conducting power to those which are commonly used in voltaic batteries or circles, in which the current is still assumed to be dependent upon contact. The simple conclusion to which the experiment should lead is, in my opinion, that the contact of iron and platinum is absolutely without any electromotive force (1835. 1859. 1889.).

1830. If the contact be made really active and effective, according to the beautiful discovery of SEEBECK, by making its temperature different to that of the other parts of the circuit, then its power of generating a current is shown (1824.). This enables us to compare the supposed power of the mere contact with that of a thermo contact; and we find that the latter comes out as infinitely greater than the former, for the former is nothing. The same comparison of mere contact and thermo contact may be made by contrasting the effect of the contact c at common temperatures, with either the contact at a or at b, either heated or cooled. Very moderate changes of temperature at these places produce instantly the corresponding current, but the mere contact at x does nothing.

1831. So also I believe that a true and philosophic and even rigid comparison may be made at x, between the assumed effect of mere contact and that of chemical action. For if the metals at x be separated, and a piece of paper moistened in dilute acid, or a solution of salt, or if only the tongue or a wet finger be applied there, then a current is caused, stronger by far than the thermo-currents before produced (1830.), passing from the iron through the introduced acid or other active fluid to the platinum. This is a case of current from chemical action without any metallic contact in the circuit on which the effect can for a moment be supposed to depend (879.); it is even a case where metallic contact is changed for chemical action, with the result, that where contact is found to be quite ineffectual, chemical action is very energetic in producing a current.

1832. It is of course quite unnecessary to say that the same experimental comparisons may be made at either of the other contacts, a or b.

1833. Admitting for the moment that the arrangement proves that the contact of platinum and iron at x has no electromotive force (1835. 1859.), then it follows also that the contact of either platinum or iron with any other metal has no such force. For if another metal, as zinc, be interposed between the iron and platinum at x, fig. 2,

no current is produced; and yet the test application of a little heat at *a* or *b*, will show by the corresponding current, that the circuit being complete will conduct any current that may tend to pass. Now that the contacts of zinc with iron and with platinum are of equal electromotive force, is not for a moment admitted by those who support the theory of contact activity; we ought therefore to have a resulting action equal to the differences of the two forces, producing a certain current. No such current is produced, and I conceive, with the admission above, that such a result proves that the contacts *iron-zinc* and *platinum-zinc* are entirely without electromotive force.

1834. Gold, silver, potassium, and copper were introduced at *x* with the like negative effect; and so no doubt might every other metal, even according to the relation admitted amongst the metals by the supporters of the contact theory (1809.). The same negative result followed upon the introduction of many other conducting bodies at the same place; as, for instance, those already mentioned as easily conducting the thermo current (1820.); and the effect proves, I think, that the contact of any of these with either iron or platinum is utterly ineffective as a source of electromotive force.

1835. The only answer which, as it appears to me, the contact theory can set up in opposition to the foregoing facts and conclusions is, to say that the solution of sulphuret of potassium in the cup D, fig. 2, acts as a metal would do (1809.), and so the effects of all the contacts in the circuit are exactly balanced. I will not stop at this moment to show that the departure with respect to electrolytes, or the fluid bodies in the voltaic pile, *from the law* which is supposed to hold good with the metals and solid conductors, though only an assumption, is still essential to the contact theory of the voltaic pile (1810. 1861.)*; nor to prove that the electrolyte is no otherwise like the metals than in having no contact electromotive force whatever. But believing that this will be very evident shortly, I will go on with the experimental results, and resume these points hereafter (1859. 1889.).

1836. The experiment was now repeated with the substitution of a bar of *nickel* for that of iron, fig. 2 (1824.), all other things remaining the same†. The circuit was again found to be a good conductor of a feeble thermo current, but utterly inefficient as a voltaic circuit when all was at the same temperature, and due precautions taken (2051.). The introduction of metals at the contact *x* was as ineffective as before (1834.); the introduction of chemical action at *x* was as striking in its influence as in the former case (1831.); all the results were, in fact, parallel to

* See Fechner's words. Philosophical Magazine, 1838, xiii. 377.

† There is another form of this experiment which I sometimes adopted, in which the cup E, fig. 2, with its contents, was dismissed, and the platinum plates in it connected together. The arrangement may then be considered as presenting three contacts of iron and platinum, two acting in one direction, and one in the other. The arrangement and the results are virtually the same as those already given. A still simpler but equally conclusive arrangement for many of the arguments, is to dismiss the iron between *a* and *b* altogether, and so have but one contact, that at *x*, to consider.

those already obtained; and if the reasoning then urged was good, it will now follow that the contact of platinum and nickel with each other, or of either with any of the different metals or solid conductors introduced at x, is entirely without electromotive force*.

1837. Many other pairs of metals were compared together in the same manner; the solution of sulphuret of potassium connecting them together at one place, and their mutual contact doing that office at another. The following are cases of this kind: iron and gold; iron and palladium; nickel and gold; nickel and palladium; platina and gold; platina and palladium. In all these cases the results were the same as those already given with the combinations of platinum and iron.

1838. It is necessary that due precaution be taken to have the arrangements in an unexceptionable state. It often happened that the first immersion of the plates gave deflections; it is, in fact, almost impossible to put two plates of the *same metal* into the solution without causing a deflection; but this generally goes off very quickly, and then the arrangement may be used for the investigation (1826.). Sometimes there is a feeble but rather permanent deflection of the needle; thus when platinum and palladium were the metals, the first effect fell and left a current able to deflect the galvanometer-needle 3°, indicating the platinum to be positive to the palladium. This effect of 3°, however, is almost nothing compared to what a mere thermo current can cause, the latter producing a deflection of 60° or more; besides which, even supposing it an essential effect of the arrangement, it is in the wrong direction for the contact theory. I rather incline to refer it to that power which platinum and other substances have of effecting combination and decomposition without themselves entering into union; and I have occasionally found that when a platinum plate has been left for some hours in a strong solution of sulphuret of potassium (1812.) a small quantity of sulphur has been deposited upon it. Whatever the cause of the final feeble current may be, the effect is too small to be of any service in support of the contact theory; while, on the other hand, it affords delicate and, therefore, strong indications in favour of the chemical theory.

1839. A change was made in the form and arrangement of the cup D, fig. 2, so as to allow of experiments with other bodies than the metals. The solution of sulphuret of potassium was placed in a shallow vessel, the platinum plate was bent so that the immersed extremity corresponded to the bottom of the vessel; on this a piece of loosely folded cloth was laid in the solution, and on that again the mineral or other substance to be compared with the platinum; the fluid being of such depth that only part of that substance was in it, the rest being clean and dry; on this portion the platinum wire, which completed the circuit, rested. The arrangement of this part of

* One specimen of nickel was, on its immersion, positive to platinum for seven or eight minutes, and then became neutral. On taking it out it seemed to have a yellowish tint on it, as if invested by a coat of sulphuret; and I suspected this piece had acted like lead (1885.) and bismuth (1895.). It is difficult to get pure and also perfectly compact nickel; and if porous, then the matter retained in the pores produces currents.

the circuit is given in section at fig. 3, where H represents a piece of galena to be compared with the platinum P.

1840. In this way galena, compact yellow copper pyrites, yellow iron pyrites, and globules of oxide of burnt iron, were compared with platinum, (the solution of sulphuret of potassium being the electrolyte used in the circuit,) and with the same results as were before obtained with metals (1829. 1833.).

1841. Experiments hereafter to be described gave arrangements in which, with the same electrolyte, sulphuret of lead was compared with gold, palladium, iron, nickel, and bismuth (1885. 1886.); also sulphuret of bismuth with platinum, gold, palladium, iron, nickel, lead, and sulphuret of lead (1894.), and always with the same result. Where no chemical action occurred there no current was formed; although the circuit remained an excellent conductor, and the contact existed by which, it is assumed in the contact theory, such a current should be produced.

1842. Instead of the strong solution, a dilute solution of the yellow sulphuret of potassium, consisting of one volume of strong solution (1812.) and ten volumes of water, was used. Plates of platinum and iron were arranged in this fluid as before (1824.); at first the iron was negative (2049.), but in ten minutes it was neutral, and the needle at 0°*. Then a weak chemical current excited at x (1831.) easily passed; and even a thermo current (1830.) was able to show its effects at the needle. Thus a strong or a weak solution of this electrolyte showed the same phenomena. By diluting the solution still further, a fluid could be obtained in which the iron was, after the first effect, permanently but feebly positive. On allowing time, however, it was found that in all such cases black sulphuret formed here and there on the iron. Rusted iron was negative to platinum (2049.) in this very weak solution, which by direct chemical action could render metallic iron positive.

1843. In all the preceding experiments the electrolyte used has been the sulphuret of potassium solution; but I now changed this for another, very different in its nature, namely, the *green nitrous acid* (1816.), which has already been shown to be an excellent conductor of electricity. Iron and platinum were the metals employed, both being in the form of wires. The vessel in which they were immersed was a tube like that formerly described (1815.); in other respects the arrangement was the same in principle as those already used (1824. 1836.). The first effect was the production of a current, the iron being positive in the acid to the platina; but this *quickly ceased*, and the galvanometer-needle came to 0°. In this state, however, the circuit could not in all things be compared with the one having the solution of sulphuret of

* Care was taken in these and the former similar cases to discharge the platinum surface of any reacting force it might acquire from the action of the previous current, by separating it from the other metals, and touching it in the liquid for an instant with another platinum plate.

potassium for its electrolyte (1824.); for although it could conduct the thermo current of antimony and bismuth in a certain degree, yet that degree was very small compared to the power possessed by the former arrangement, or to that of a circle in which the nitrous acid was between two platinum plates (1816.). This remarkable retardation is consequent upon the assumption by the iron of that peculiar state which SCHŒNBEIN has so well described and illustrated by his numerous experiments and investigations. But though it must be admitted that the iron in contact with the acid is in a peculiar state (1951. 2001. 2033.), yet it is also evident that a circuit consisting of platinum, iron, peculiar iron, and nitrous acid, does not cause a current though it have sufficient conducting power to carry a thermo current.

1844. But if the contact of platinum and iron has an electromotive force, why does it not produce a current? The application of heat (1830.), or of a little chemical action (1831.) at the place of contact, does produce a current, and in the latter case a strong one. Or if any other of the contacts in the arrangement can produce a current, why is not that shown by some corresponding effect? The only answers are, to say, that the peculiar iron has the same electromotive properties and relations as platinum, or that the nitrous acid is included under the same law with the metals (1809. 1835.); and so the sum of the effects of all the contacts in the circuit is nought, or an exact balance of forces. That the iron is like the platinum in having no electromotive force at its contacts without chemical action, I believe; but that it is unlike it in its electrical relations, is evident from the difference between the two in strong nitric acid, as well as in weak acid; from their difference in the power of transmitting electric currents to either nitric acid or sulphuret of potassium, which is very great; and also by other differences. That the nitrous acid is, as to the power of its contacts, to be separated from other electrolytes and classed with the metals in what is, with them, only an assumption, is a gratuitous mode of explaining the difficulty, which will come into consideration, with the case of sulphuret of potassium, hereafter (1835. 1859. 1889. 2060.).

1845. To the electro-chemical philosopher, the case is only another of the many strong instances, showing that where chemical action is absent in the voltaic circuit, there no current can be formed; and that whether solution of sulphuret of potassium or nitrous acid be the electrolyte or connecting fluid used, still the results are the same, and contact is shown to be inefficacious as an active electromotive condition.

1846. I need not say that the introduction of different metals between the iron and platinum at their point of contact, produced no difference in the results (1833. 1834.) and caused no current; and I have said that heat and chemical action applied there produced their corresponding effects. But these parallels in action and non-action show the identity in nature of this circuit, (notwithstanding the production of the surface of peculiar iron on that metal,) and that with solution of sulphuret of potassium: so that all the conclusions drawn from it apply here; and if that case

ultimately stand firm as a proof against the theory of contact force, this will stand also.

1847. I now used oxide of iron and platinum as the extremes of the solid part of the circuit, and the nitrous acid as the fluid; i. e. I heated the iron wire in the flame of a spirit-lamp, covering it with a coat of oxide in the manner recommended by SCHŒNBEIN in his investigations, and then used it instead of the clean iron (1843.). The oxide of iron was at first in the least degree positive, and then immediately neutral. This circuit, then, like the former, gave no current at common temperatures; but it differed much from it in conducting power, being a very excellent conductor of a thermo current, the oxide of iron not offering that obstruction to the passage of the current which the peculiar iron did (1843. 1844.). Hence scale oxide of iron and platinum produce no current by contact, the third substance in the proof circuit being nitrous acid; and so the result agrees with that obtained in the former case, where that third substance was solution of sulphuret of potassium.

1848. In using nitrous acid it is necessary that certain precautions be taken, founded on the following effect. If a circuit be made with the green nitrous acid, platinum wires, and a galvanometer, in a few seconds all traces of a current due to first disturbances will disappear; but if one wire be raised into the air and instantly returned to its first position, a current is formed, and that wire is negative, across the electrolyte, to the other. If one wire be dipped only a small distance into the acid, as for instance one fourth of an inch, then the raising that wire not more than one eighth of an inch and instantly restoring it, will produce the same effect as before. The effect is due to the evaporation of the nitrous acid from the exposed wire (1937.). I may perhaps return to it hereafter, but wish at present only to give notice of the precaution that is required in consequence, namely, to retain the immersed wires undisturbed during the experiment.

1849. Proceeding on the facts made known by SCHŒNBEIN respecting the relation of iron and nitric acid, I used that acid as the fluid in a voltaic circuit formed with iron and platinum. Pure nitric acid is so deficient in conducting power (1817.) that it may be supposed capable of stopping any current due to the effect of contact between the platinum and iron; and it is further objectionable in these experiments, because, acting feebly on the iron, it produces a chemically excited current, which may be considered as mingling its effect with that of contact: whereas the object at present is, by excluding such chemical action, to lay bare the influence of contact alone. Still the results with it are consistent with the more perfect ones already described; for in a circuit of iron, platinum, and nitric acid, the joint effects of the chemical action on the iron and the contact of iron and platinum, being to produce a current of a certain constant force indicated by the galvanometer, a little chemical action, brought into play where the iron and platinum were in contact as before (1831.), produced a

current far stronger than that previously existing. If then, from the weaker current, the part of the effect due to chemical action be abstracted, how little room is there to suppose that any effect is due to the contact of the metals.

1850. But a *red nitric acid* with platinum plates conducts a thermo current well, and will do so even when considerably diluted (1818.). When such red acid is used between iron and platinum, the conducting power is such, that one half of the permanent current can be overcome by a counter thermo current of bismuth and antimony. Thus a sort of comparison is established between a thermo current on the one hand, and a current due to the joint effects of chemical action on iron and contact of iron and platinum on the other. Now considering the admitted weakness of a thermo current, it may be judged what the strength of that part of the second current due to contact can, at the utmost, be; and how little it is able to account for the strong currents produced by ordinary voltaic combinations.

1851. If for a clean iron wire one oxidized in the flame of a spirit-lamp be used, being associated with platinum in pure strong nitric acid, there is a feeble current, the oxide of iron being positive to the platinum, and the facts mainly as with iron. But the further advantage is obtained of comparing the contact of strong and weak acid with this oxidized wire. If one volume of the strong acid and four volumes of water be mixed, this solution may be used, and there is even less deflection than with the strong acid: the iron side is now not sensibly active, except the most delicate means be used to observe the current. Yet in both cases if a chemical action be introduced in place of the contact, the resulting current passes well, and even a thermo current can be made to show itself as more powerful than any due to contact.

1852. In these cases it is safest to put the whole of the oxidized iron under the surface and connect it in the circle by touching it with a platinum wire; for if the oxidized iron be continued through from the acid to the air, it is almost certain to suffer from the joint action of the acid and air at their surface of contact.

1853. I proceeded to use a fluid differing from any of the former: this was solution of *potassa*, which has already been employed by De la Rive (1823.) with iron and platina, and which when strong has been found to be a substance conducting so well, that even a thermo current could pass it (1819.), and therefore fully sufficient to show a contact current, if any such exists.

1854. Yet when a strong solution of this substance was arranged with silver and platinum, (bodies differing sufficiently from each other when connected by nitric or muriatic acid,) as in the former cases, a very feeble current was produced, and the galvanometer-needle stood nearly at zero. The contact of these metals therefore did not appear to produce a sensible current; and, as I fully believe, because no electromotive power exists in such contact. When that contact was exchanged for a very feeble chemical action, namely, that produced by interposing a little piece of paper

moistened in dilute nitric acid (1831.), a current was the result. So here, as in the many former cases, the arrangement with a little chemical action and no metallic contact produces a current, but that without the chemical action and with the metallic contact produces none.

1855. Iron or nickel associated with platinum in this strong solution of potassa was positive. The force of the produced current soon fell, and after an hour or so was very small. Then annulling the metallic contact at *x*, fig. 2, and substituting a feeble chemical action there, as of dilute nitric acid, the current established by the latter would pass and show itself. Thus the cases are parallel to those before mentioned (1849, &c.), and show how little contact alone could do, since the effect of the conjoint contact of iron and platinum and chemical action of potash and iron were very small as compared with the contrasted chemical action of the dilute nitric acid.

1856. Instead of a strong solution of potassa, a much weaker one consisting of one volume of strong solution and six volumes of water was used, but the results with the silver and platinum were the same: no current was produced by the metallic contact as long as that only was left for exciting cause, but on substituting a little chemical action in its place (1831.), the current was immediately produced.

1857. Iron and nickel with platinum in the weak solution also produced similar results, except that the positive state of these metals was rather more permanent than with the strong solution. Still it was so small as to be out of all proportion to what was to be expected according to the contact theory.

1858. Thus these different contacts of metals and other well-conducting solid bodies prove utterly inefficient in producing a current, as well when solution of potassa is the third or fluid body in the circuit, as when that thirdbody is either solution of sulphuret of potassium, or hydrated nitrous acid, or nitric acid, or mixed nitric and nitrous acids. Further, all the arguments respecting the inefficacy of the contacts of bodies interposed at the junction of the two principal solid substances, which were advanced in the case of the sulphuret of potassium solution (1833.), apply here with potassa; as they do indeed in every case of a conducting circuit where the interposed fluid is without chemical action and no current is produced. If a case could be brought forward in which the interposed fluid is without action, is yet a sufficiently good conductor, and a current *is* produced; then, indeed, the theory of contact would find evidence in its favour, which, as far as I can perceive, could not be overcome. I have most anxiously sought for such a case, but cannot find one (1798.).

1859. The argument is now in a fit state for the resumption of that important point before adverted to (1835. 1844.), which, if truly advanced by an advocate for the

contact theory, would utterly annihilate the force of the previous experimental results, though it would not enable that theory to give a reason for the activity of, and the existence of a current in, the pile: but which, if in error, would leave the contact theory utterly defenceless and without foundation.

1860. A supporter of the contact theory may say that the various conducting electrolytes used in the previous experiments are like the metals; i. e. that they have an electromotive force at their points of contact with the metals and other solid conductors employed to complete the circuit; but that this is of such consistent strength at each place of contact, that, in a complete circle, the sum of the forces is 0 (1809.). The actions at the contacts are tense electromotive actions, but balanced, and so no current is produced. But what experiment is there to support this statement? where are the measured electromotive results proving it (1808.)? I believe there are none.

1861. The contact theory after assuming that mere contacts of dissimilar substances have electromotive powers, further assumes a difference between metals and liquid conductors (1810.) without which it is impossible that the theory can explain the current in the voltaic pile: for whilst the contact effects in a metallic circuit are assumed to be always perfectly balanced, it is also assumed that the contact effects of the electrolytes or interposed fluid with the metals are not balanced, but are so far removed from anything like an equilibrium, as to produce most powerful currents, even the strongest that a voltaic pile can produce. If so, then why should the solution of sulphuret of potassium be an exception? it is quite unlike the metals: it does not appear to conduct without decomposition; it is an excellent electrolyte, and an excellent *exciting* electrolyte in proper cases (1880.), producing most powerful currents when it acts chemically; it is in all these points quite unlike the metals, and, in its action, like any of the acid or saline exciting electrolytes commonly used. How then can it be allowed that, without a single direct experiment, and solely for the purpose of avoiding the force of those which are placed in opposition, we should suppose it to leave its own station amongst the electrolytes, and class with the metals; and that too, in a point of character which, even with them, is as yet a mere assumption (1809.).

1862. But it is not with the sulphuret of potassium alone that this freedom must be allowed; it must be extended to the nitrous acid (1843. 1847.), to the nitric acid (1849, &c.), and even to the solution of potash (1854.); all these being of the class of electrolytes, and yet exhibiting no current in circuits where they do not occasion chemical action. Further, this exception must be made for *weak solutions* of sulphuret of potassium (1842.) and of potassa (1856.), for they exhibit the same phenomena as the stronger solutions. And if the contact theorists claim it for these weak solutions, then how will they meet the case of weak nitric acid which is not similar in its action on iron to strong nitric acid (1977.), but can produce a powerful current?

1863. The chemical philosopher is embarrassed by none of these difficulties; for he first, by a simple direct experiment, ascertains whether any of the two given substances in the circuit are active chemically on each other. If they are, he expects and finds the corresponding current; if they are not, he expects and he finds no current, though the circuit be a good conductor and he look carefully for it (1829.).

1864. Again; taking the case of iron, platina, and solution of sulphuret of potassium, there is no current; but for iron substitute zinc, and there is a powerful current. I might for zinc substitute copper, silver, tin, cadmium, bismuth, lead, and other metals; but I take zinc, because its sulphuret dissolves and is carried off by the solution, and so leaves the case in a very simple state: the fact, however, is as strong with any of the other metals. Now if the contact theory be true, and if the iron, platina, and solution of sulphuret of potassium give contacts which are in perfect equilibrium as to their electromotive force, then why does changing the iron for zinc destroy the equilibrium? Changing one metal for another in a metallic circuit causes no alteration of this kind: nor does changing one substance for another among the great number of bodies which, as solid conductors, may be used to form conducting (but chemically inactive) circuits (1867, &c.). If the solution of sulphuret of potassium is to be classed with the metals as to its action in the experiments I have quoted (1825, &c.), then, how comes it to act quite unlike them, and with a power equal to the *best* of the other class, in the new cases of zinc, copper, silver, &c. (1882. 1885, &c.).

1865. This difficulty, as I conceive, must be met, on the part of the contact theorists, by a new assumption, namely, that this fluid sometimes acts as the best of the metals, or first class of conductors, and sometimes as the best of the electrolytes or second class. But surely this would be far too loose a method of philosophizing in an experimental science (1889.); and further, it is most unfortunate for such an assumption, that this second condition or relation of it never comes on by itself, so as to give us a pure case of a current from contact alone; it never comes on *without* that chemical action to which the chemist so simply refers all the current which is then produced.

1866. It is unnecessary for me to say that the same argument applies with equal force to the cases where nitrous acid, nitric acid, and solution of potash are used; and it is supported with equal strength by the results which they have given (1843. 1849. 1853.).

1867. It may be thought that it was quite unnecessary, but in my desire to establish contact electromotive force, to do which I was at one time very anxious, I made many circuits of three substances, including a galvanometer, all being conductors, with the hope of finding an arrangement which, without chemical action, should produce a current. The number and variety of these experiments may be understood

from the following summary; in which metals, plumbago, sulphurets and oxides, all being conductors even of a thermo current, were thus combined in various ways:

1. Platinum.
2. Iron.
3. Zinc.
4. Copper.
5. Plumbago.
6. Scale oxide of iron.
7. Native peroxide of manganese.
8. Native gray sulphuret of copper.
9. Native iron pyrites.
10. Native copper pyrites.
11. Galena.
12. Artificial sulphuret of copper.
13. Artificial sulphuret of iron.
14. Artificial sulphuret of bismuth.

1 and 2 with 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, in turn.
1 and 3 with 5, 6, 7, 8, 9, 10, 11, 12, 13, 14.
1 and 5 with 6, 7, 8, 9, 10, 11, 12, 13, 14.
3 and 6 with 7, 8, 9, 10, 11, 12, 13, 14.
4 and 5 with 6, 7, 8, 9, 10, 11, 12, 13, 14.
4 and 6 with 7, 8, 9, 10, 11, 12, 13, 14.
4 and 7 with 8, 9, 10, 11, 12, 13, 14.
4 and 8 with 9, 10, 11, 12, 13, 14.
4 and 9 with 10, 11, 12, 13, 14.
4 and 10 with 11, 12, 13, 14.
4 and 11 with 12, 13, 14.
4 and 12 with 13, 14.
4 and 13 with 14.
1 and 4 with 12.

1868. MARIANINI states from experiment that copper is positive to sulphuret of copper*: with the Voltaists, according to the same philosopher, sulphuret of copper is positive to iron (1878.), and with them also iron is positive to copper. These three bodies therefore ought to give a most powerful circle: but on the contrary, whatever sulphuret of copper I have used, I have found not the slightest effect from such an arrangement.

1869. As peroxide of lead is a body causing a powerful current in solution of sulphuret of potassium, and indeed in every case of a circuit where it can give up part of its oxygen, I thought it reasonable to expect that its contact with metals would produce a current, if contact ever could. A part of that which had been prepared (1822.), was therefore well dried, which is quite essential in these cases, and formed into the following combinations:

Platinum.	Zinc.	Peroxide of lead.
Platinum.	Lead.	Peroxide of lead.
Platinum.	Cadmium.	Peroxide of lead.
Platinum.	Iron.	Peroxide of lead.

* Memoria della Società Italiana in Modena, 1827, xxi. 224.

Of these varied combinations, not one gave the least signs of a current, provided differences of temperature were excluded; though in every case the circle formed was, as to conducting power, perfect for the purpose, i. e. able to conduct even a very weak thermo current.

1870. In the contact theory it is not therefore the metals alone that must be assumed to have their contact forces so balanced as to produce, in any circle of them, an effect amounting to nothing (1809.); but all solid bodies that are able to conduct, whether they be forms of carbon, or oxides, or sulphurets, must be included in the same category. So also must the electrolytes already referred to, namely, the solutions of sulphuret of potassium and potash, and nitrous and nitric acids, in every case where they do not act chemically. In fact *all conductors* that do not act chemically in the circuit must be assumed, by the contact theory, to be in this condition, until a case of voltaic current without chemical action is produced (1858.).

1871. Then, even admitting that the results obtained by VOLTA and his followers with the electrometer prove that mere contact has an electromotive force and can produce an effect, surely all experience with contact alone goes to show that the electromotive forces in a circuit are always balanced. How else is it likely that the abovenamed most varied substances should be found to agree in this respect? unless indeed it be, as I believe, that all substances agree in this, of having no such power at all. If so, then where is the source of power which can account by the theory of contact for the current in the voltaic pile? If they are not balanced, then where is the sufficient case of contact alone producing a current? or where are the numerical data which indicate that such a case can be (1808. 1868.)? The contact philosophers are bound to produce, not a case where the current is infinitesimally small, for such cannot account for the current of the voltaic pile, and will always come within the debatable ground which DE LA RIVE has so well defended, but a case and data of such distinctness and importance as may be worthy of opposition to the numerous cases produced by the chemical philosopher (1892.); for without them the contact theory as applied to the pile appears to me to have *no* support, and, as it asserts contact electromotive force even *with* the balanced condition, to be almost without foundation.

1872. To avoid these and similar conclusions, the contact theory must bend about in the most particular and irregular way. Thus the contact of solution of sulphuret of potassium with iron must be considered as balanced by the joint force of its contact with platinum, and the contact of iron and platinum with each other; but changing the iron for lead, then the contact of the sulphuret with the latter metal is no longer balanced by the other two contacts, it has all of a sudden changed its relation: after a few seconds, when a film of sulphuret has been formed by the chemical action, then the current ceases, though the circuit be a good conductor (1885.); and

now it must be assumed that the solution has acquired its first relation to the metals and to the sulphuret of lead, and gives an equilibrium condition of the contacts in the circle.

1873. So also with this sulphuretted solution and with potassa, dilution must, by the theory, be admitted as producing *no change* in the character of the contact force; but with nitric acid it, on the contrary, must be allowed to change the character of the force greatly (1977.). So again acids and alkalies (as potassa) in the cases where currents are produced by them, as with zinc and platinum for instance, must be assumed as giving the preponderance of electromotive force on the same side, though these are bodies which might have been expected to give opposite currents, since they differ so much in their nature.

1874. Every case of a current is obliged to be met, on the part of the contact advocates, by assuming powers at the points of contact, in *the particular case*, of such proportionate strengths as will consist with the results obtained, and the theory is made to bend about (1956. 1992. 2006. 2014. 2063.), having no general relation for the acids or alkalies, or other electrolytic solution used. The result therefore comes to this: The theory can predict nothing regarding the results; it is accompanied by no case of a voltaic current produced without chemical action, and in those associated with chemical action, it bends about to suit the real results, these contortions being exactly parallel to the variations which the pure chemical force, by experiment, indicates.

1875. In the midst of all this, how simply does the chemical theory meet, include, combine, and even predict, the numerous experimental results! When there is a current there is also chemical action; when the action ceases, the current stops (1882. 1885. 1894.); the action is determined either at the anode or the cathode, according to circumstances (2039. 2041.), and the direction of the current is invariably associated with the direction in which the active chemical forces oblige the anions and cations to move in the circle (962. 2052.).

1876. Now when in conjunction with these circumstances it is considered, that the many arrangements without chemical action (1825, &c.) produce no current; that those with chemical action almost always produce a current; that hundreds occur in which chemical action without contact produces a current (2017, &c.); and that as many with contact but without chemical action (1867.) are known and are inactive; how can we resist the conclusion, that the powers of the voltaic battery originate in the exertion of chemical force?

¶ iii. *Active circles excited by solution of sulphuret of potassium.*

1877. In 1812 Davy gave an experiment to show, that of two different metals, copper and iron, that having the strongest attraction for oxygen was positive in oxidizing solutions, and that having the strongest attraction for sulphur was positive in

sulphuretting solutions*. In 1827 De la Rive quoted several such inversions of the states of two metals, produced by using different solutions, and reasoned from them, that the mere contact of the metals could not be the cause of their respective states, but that the chemical action of the liquid produced these states†.

1878. In a former paper I quoted Sir Humphry Davy's experiment (943.), and gave its result as a proof that the contact of the iron and copper could not originate the current produced; since when a dilute acid was used in place of the sulphuret, the current was reverse in direction, and yet the contact of the metals remained the same. M. Marianini‡ adds, that copper will produce the same effect with tin, lead, and even zinc; and also that silver will produce the same results as copper. In the case of copper he accounts for the effect by referring it to the relation of the iron and the new body formed on the copper, the latter being, according to Volta, positive to the former§. By his own experiment the same substance was negative to the iron across the same solution‖.

1879. I desire at present to resume the class of cases where a solution of sulphuret of potassium is the liquid in a voltaic circuit; for I think they give most powerful proof that the current in the voltaic battery cannot be produced by contact, but is due altogether to chemical action.

1880. The solution of sulphuret of potassium (1812.) is a most excellent conductor of electricity (1814.). When subjected between platinum electrodes to the decomposing power of a small voltaic battery, it readily gave pure sulphur at the anode, and a little gas, which was probably hydrogen, at the cathode. When arranged with platinum surfaces so as to form a Ritter's secondary pile, the passage of a feeble primary current, for a few seconds only, makes this secondary battery effective in causing a counter current; so that, in accordance with electrolytic conduction (923. 1343.), it probably does not conduct without decomposition, or if at all, its point of electrolytic intensity (966. 983.) must be very low. Its exciting action (speaking on the chemical theory) is either the giving an anion (sulphur) to such metallic and other bodies as it can act upon, or, in some cases, as with the peroxides of lead and manganese, and the protoxide of iron (2046.), the abstraction of an anion *from* the body in contact with it, the current produced being in the one or the other direction accordingly. Its chemical affinities are such, that in many cases its anion goes to that metal, of a pair of metals, which is left untouched when the usual exciting electrolytes are employed; and so a beautiful inversion of the current in relation to the metals is obtained; thus, when copper and nickel are used with it, the anion goes to the copper; but when the same metals are used with the ordinary electrolytic fluids, the anion goes to the nickel. Its excellent conducting power renders

* Elements of Chemical Philosophy, p. 148.

† Annales de Chimie, 1828, xxxvii. 231–237; xxxix. 299.

‡ Memorie della Società Italiana in Modena, 1837, xxi. p. 224.

§ Ibid. p. 219. ‖ Ibid. p. 224.

the currents it can excite very evident and strong; and it should be remembered that the strength of the resulting currents, as indicated by the galvanometer, depends jointly upon the energy (not the mere quantity) of the exciting action called into play, and the conductive ability of the circuit through which the current has to run. The value of this exciting electrolyte is increased for the present investigation, by the circumstance of its giving, by its action on the metals, resulting compounds, some of which are insoluble, whilst others are soluble; and, of the insoluble results, some are excellent conductors, whilst others have no conducting power at all.

1881. The experiments to be described were made generally in the following manner. Wires of platinum, gold, palladium, iron, lead, tin, and the other malleable metals, about one twentieth of an inch in diameter and six inches long, were prepared. Two of these being connected with the ends of the galvanometer-wires, were plunged at the same instant into the solution of sulphuret of potassium in a test-glass, and kept there without agitation (1919.), the effects at the same time being observed. The wires were in every case carefully cleansed with fresh fine sand-paper and a clean cloth; and were sometimes even burnished by a glass rod, to give them a smooth surface. Precautions were taken to avoid any difference of temperature at the junctions of the different metals with the galvanometer-wires.

1882. *Tin and platinum.*—When tin was associated with platinum, gold, or, I may say, any other metal which is chemically inactive in the solution of the sulphuret, a strong electric current was produced, the tin being positive to the platinum through the solution, or, in other words, the current being from the tin through the solution to the platinum In a very short time this current fell greatly in power, and in ten minutes the galvanometer-needle was nearly at 0°. On then endeavouring to transmit the antimony-bismuth thermo current (1825.) through the circuit, it was found that it could not pass, the circle having lost its conducting power. This was the consequence of the formation on the tin of an insoluble, investing, non-conducting sulphuret of that metal; the non-conducting power of the body formed is not only evident from the present result, but also from a former experiment (1821.).

1883. Marianini thinks it is possible that (in the case of copper, at least (1878.), and so, I presume, for all similar cases, for surely one law or principle should govern them,) the current is due to the contact force of the sulphuret formed. But that application is here entirely excluded; for how can a *non-conducting* body form a current, either by contact or in any other way? No such case has ever been shown, nor is it in the nature of things; so that it cannot be the contact of the sulphuret that here causes the current; and if not in the present, why in any case? for nothing happens here that does not happen in any other instance of a current produced by the same exciting electrolyte.

1884. On the other hand, how beautiful a proof the result gives in confirmation of the chemical theory! Tin can take sulphur from the electrolyte to form a sulphuret; and whilst it is doing so, and in proportion to the degree in which it is doing so, it

produces a current; but when the sulphuret which is formed, by investing the metal, shuts off the fluid and prevents further chemical action, then the current ceases also. Nor is it *necessary* that it should be a non-conductor for this purpose, for conducting sulphurets will perform the same office (1885. 1894.), and bring about the same result. What, then, can be more clear, than that whilst the sulphuret is *being formed* a current is produced, but that when formed its mere contact can do nothing towards such an effect?

1885. *Lead.*—This metal presents a fine result in the solution of sulphuret of potassium. Lead and platinum being the metals used, the lead was at first highly positive, but in a few seconds the current fell, and in two minutes the galvanometer-needle was at 0°. Still the arrangement conducted a feeble thermo current extremely well, the conducting power not having disappeared, as in the case of tin; for the investing sulphuret of lead is a conductor (1820.). Nevertheless, though a conductor, it could stop the further chemical action; and that ceasing, the current ceased also.

1886. Lead and gold produced the same effect. Lead and palladium the same. Lead and iron the same, except that the circumstances respecting the tendency of the latter metal under common circumstances to produce a current from the electrolyte to itself, have to be considered and guarded against (1826. 2049.). Lead and nickel also the same. In all these cases, when the lead was taken out and washed, it was found beautifully invested with a thin polished pellicle of sulphuret of lead.

1887. With lead, then, we have a *conducting* sulphuret formed, but still there is no sign that its contact can produce a current, any more than in the case of the *non-conducting* sulphuret of tin (1882.). There is no new or additional action produced by this *conducting* body; there was no deficiency of action with the former *non-conducting* product; both are alike in their results, being, in fact, essentially alike in their relation to that on which the current really depends, namely, an active chemical force. A piece of lead put *alone* into the solution of sulphuret of potassium, has its surface converted into sulphuret of lead, the proof thus being obtained, even when the current cannot be formed, that there is a force (chemical) present and active under such circumstances; and such force can produce a current of chemical force when the circuit form is given to the arrangement. The force at the place of excitement shows itself, both by the formation of sulphuret of lead and the production of a current. In proportion as the formation of the one decreases the production of the other diminishes, though all the bodies produced are conductors, and contact still remains to perform any work or cause any effect to which it is competent.

1888. It may perhaps be said that the current is due to the contact between the solution of sulphuret and the lead, (or tin, as the case may be,) which occurs at the beginning of the experiment; and that when the action ceases, it is because a new body, the sulphuret of lead, is introduced into the circuit, the various contacts being then balanced in their force. This would be to fall back upon the assumption before

resisted (1861. 1865. 1872.), namely, that the solution may class with metals and such-like bodies, giving balanced effects of contact in relation to *some* of these bodies, as in this case, to the sulphuret of lead produced, but not with *others*, as the lead itself; both the lead and its sulphuret being in the same category as the metals generally (1809. 1870.).

1889. The utter improbability of this as a natural effect, and the absence of all experimental proof in support of it, have been already stated (1861. 1871.), but one or two additional reasons against it now arise. The state of things may perhaps be made clearer by a diagram or two, in which assumed contact forces may be assigned, in the absence of all experimental expression, without injury to the reasoning. Let fig. 4. Plate III. represent the electromotive forces of a circle of platinum, iron, and solution of sulphuret of potassium; or platinum, nickel, and solution of sulphuret; cases in which the forces are, according to the contact theory, balanced (1860.). Then fig. 5 may represent the circle of platinum, lead, and solution of sulphuret, which does produce a current, and, as I have assumed, with a resulting force of 11 ⟶. This in a few minutes becomes quiescent, i. e. the current ceases, and fig. 6 may represent this new case according to the contact theory. Now is it at all likely that by the intervention of sulphuret of lead at the contact *c*, fig. 5, and the production of two contacts *d* and *e*, fig. 6, such an enormous change of the contact force suffering alteration should be made as from 10 to 21? the intervention of the same sulphuret either at *a* or *b* (1834. 1840.) being able to do nothing of the kind, for the sum of the force of the two new contacts is in that case exactly equal to the force of the contact which they replace, as is proved by such interposition making no change in the effects of the circle (1867. 1840.). If therefore the intervention of this body between *lead* and platinum at *a*, or between solution of *sulphuret of potassium* and platinum at *b* (fig. 5.) causes no change, these cases including its contact with both lead and the solution of sulphuret, is it at all probable that its intervention between these two bodies at *c* should make a difference equal to double the amount of force previously existing, or indeed any difference at all?

1890. Such an alteration as this in the sum assigned as the amount of the forces belonging to the sulphuret of lead by virtue of its two places of contact, is equivalent I think to saying that it partakes of the anomalous character already supposed to belong to certain fluids, namely, of sometimes giving balanced forces in circles of good conductors, and at other times not (1865.).

1891. Even the metals themselves must in fact be forced into this constrained condition; for the effect at a point of contact, if there be *any at all*, must be the result of the *joint* and *mutual actions* of the bodies in contact. If therefore in the circuit, fig. 5, the contact forces are not balanced, it must be because of the deficient *joint* action of the lead and solution at *c**. If the metal and fluid were to act in their

* My numbers are assumed, and if other numbers were taken, the reasoning might be removed to contact *b*, or even to contact *a*, but the end of the argument would in every case be the same.

proper character, and as iron or nickel would do in the place of the lead, then the force there would be — 21, whereas it is less, or according to the assumed numbers only ←— 10. Now as there is no reason why the lead should have any superiority assigned to it over the solution, since the latter can give a balanced condition amongst good conductors in its proper situation as well as the former; how can this be, unless lead possess that strange character of sometimes giving equipoised contacts, and at other times not (1865.).

1892. If that be true of lead, it must be true of all the metals which, with this sulphuretted electrolyte, give circles producing currents; and this would include bismuth, copper, antimony, silver, cadmium, zinc, tin, &c. &c. With other electrolytic fluids iron and nickel would be included, and even gold, platinum, palladium; in fact all the bodies that can be made to yield in any way active voltaic circuits. Then is it possible that this can be true, and yet not a single combination of this extensive class of bodies be producible that can give the current without chemical action (1867.), considered not as a result, but as a known and pre-existing force?

1893. I will endeavour to avoid further statement of the arguments, but think myself bound to produce (1799.) a small proportion of the enormous body of facts which appear to me to bear evidence all in one direction.

1894. *Bismuth.*—This metal when associated with platinum, gold, or palladium in solution of the sulphuret of potassium, gives active circles, the bismuth being positive. In the course of less than half an hour the current ceases; but the circuit is still an excellent conductor of thermo currents. Bismuth with iron or nickel produces the same final result with the reservation before made (1826.). Bismuth and lead give an active circle; at first the bismuth is positive; in a minute or two the current ceases, but the circuit still conducts the thermo current well.

1895. Thus whilst sulphuret of bismuth is in the act of formation the current is produced; when the chemical action ceases the current ceases also; though contact continues and the sulphuret be a good conductor. In the case of bismuth and lead the chemical action occurs at both sides, but is most energetic at the bismuth, and the current is determined accordingly. Even in that instance the cessation of chemical action causes the cessation of the current.

1896. In these experiments with *lead* and *bismuth* I have given their associations with platinum, gold, palladium, iron, and nickel; because, believing in the first place that the results prove all current to depend on chemical action, then, the quiescent state of the resulting or final circles shows that the contacts of these metals in their respective pairs are *without force* (1829.): and upon that again follows the passive condition of all those contacts which can be produced by interposing other conducting bodies between them (1833.); an argument that need not again be urged.

1897. *Copper.*—This substance being associated with platinum, gold, iron, or any metal chemically inactive in the solution of sulphuret, gives an active circle, in which the copper is positive through the electrolyte to the other metal. The action, though

it falls, does not come to a close as in the former cases, and for these simple reasons; that the sulphuret formed is not compact but porous, and does not adhere to the copper, but separates from it in scales. Hence results a continued renewal of the chemical action between the metal and electrolyte, and a continuance of the current. If after a while the copper plate be taken out and washed, and dried, even the wiping will remove part of the sulphuret in scales, and the nail separates the rest with facility. Or if a copper plate be left in abundance of the solution of sulphuret, the chemical action *continues*, and the coat of sulphuret of copper becomes thicker and thicker.

1898. If, as Marianini has shown*, a copper plate which has been dipped in the solution of sulphuret, be removed before the coat formed is so thick as to break up from the metal beneath, and be washed and dried, and then replaced, in association with platinum or iron, in the solution, it will at the first be neutral, or, as is often the case, negative (1827. 1838.) to the other metal, a result quite in opposition to the idea, that the mere presence of the sulphuret on it could have caused the former powerful current and positive state of the copper (1897. 1878.). A further proof that it is not the mere *presence*, but the *formation*, of the sulphuret which causes the current, is, that, if the plate be left long enough for the solution to penetrate the investing crust of sulphuret of copper and come into activity on the metal beneath, then the plate becomes active, and a current is produced.

1899. I made some sulphuret of copper, by igniting thick copper wire in a Florence flask or crucible in abundance of vapour of sulphur. The body produced is in an excellent form for these experiments, and a good conductor; but it is not without action on the sulphuretted solution, from which it can take more sulphur, and the consequence is, that it is positive to platinum or iron in such a solution. If such sulphuret of copper be left long in the solution, and then be washed and dried, it will generally acquire the final state of sulphuration, either in parts or altogether, and also be inactive, as the sulphuret formed on the copper was before (1898.); i. e. when its chemical action is exhausted, it ceases to produce a current.

1900. *Native gray sulphuret of copper* has the same relation to the electrolyte: it takes sulphur from it and is raised to a higher state of combination; and, as it is also a conductor (1820.), it produces a current, being itself positive so long as the action continues.

1901. But when the copper is *fully sulphuretted*, then all these actions cease; though the sulphuret be a conductor, the contacts still remain, and the circle can carry with facility a feeble thermo current. This is not only shown by the quiescent cases just mentioned (1898.), but also by the utter inactivity of platinum and *compact yellow copper pyrites*, when conjoined by this electrolyte, as shown in a former part of this paper (1840.)

1902. *Antimony*.—This metal, being put alone into a solution of sulphuret of potassium, is acted on, and a sulphuret of antimony formed, which does not adhere

* Memorie della Società Italiana in Modena, 1837, xxi. 224.

strongly to the metal, but wipes off. Accordingly if a circle be formed of antimony, platinum, and the solution, the antimony is positive in the electrolyte, and a powerful current is formed, which continues. Here then is another beautiful variation of the conditions under which the chemical theory can so easily account for the effects, whilst the theory of contacts cannot. The sulphuret produced in this case is a non-conductor whilst in the solid state (402.); it cannot therefore be that any contact of this sulphuret can produce the current; in that respect it is like the sulphuret of tin (1882.). But that circumstance does not stop the occurrence of the chemical current; for, as the sulphuret forms a porous, instead of a continuous crust, the electrolyte has access to the metal and the action goes on.

1903. *Silver.*—This metal associated with platinum, iron, or other metals inactive in this electrolyte, is strongly positive, and gives a powerful continuous current. Accordingly, if a plate of silver, coated with sulphuret by the simple action of the solution, be examined, it will be found that the crust is brittle and broken, and separates almost spontaneously from the metal. In this respect, therefore, silver and copper are alike, and the action consequently continues in both cases; but they differ in the sulphuret of silver being a nonconductor (434.) for these feeble currents, and, in that respect, this metal is analogous to antimony (1902.).

1904. *Cadmium.*—Cadmium with platinum, gold, iron, &c., gives a powerful current in the solution of sulphuret, and the cadmium is positive. On several occasions this current continued for two or three hours or more; and at such times, the cadmium being taken out, washed, and wiped, the sulphuret was found to separate easily in scales on the cloth used.

1905. Sometimes the current would soon cease; and then the circle was found not to conduct the thermo current (1813.). In these cases, also, on examining the cadmium, the coat of sulphuret was strongly adherent, and this was more especially the case when prior to the experiment the cadmium, after having been cleaned, was burnished by a glass rod (1881.). Hence it appears that the sulphuret of this metal is a non-conductor, and that its contact could not have caused the current (1883.) in the manner MARIANINI supposes. All the results it supplies are in perfect harmony with the chemical theory and adverse to the contact theory.

1906. *Zinc.*—This metal, with platinum, gold, iron, &c., and the solution of sulphuret, produces a very powerful current, and is positive through the solution to the other metal. The current was permanent. Here another beautiful change in the circumstances of the general experiment occurs. Sulphuret of zinc is a non-conductor of electricity (1821.), like the sulphurets of tin, cadmium, and antimony; but then it is soluble in the solution of sulphuret of potassium; a property easily ascertainable by putting a drop of solution of zinc into a portion of the electrolytic solution, and first stirring them a little, by which abundance of sulphuret of zinc will be formed; and then stirring the whole well together, when it will be redissolved. The consequence of this solubility is, that the zinc when taken out of the solution is perfectly free from

investing sulphuret of zinc. Hence, therefore, a very sufficient reason, on the chemical theory, why the action should go on. But how can the theory of contact refer the current to any contact of the metallic sulphuret, when that sulphuret is, in the first place a non-conductor, and, in the next, is dissolved and carried off into the solution at the moment of its formation?

1907. Thus all the phenomena with this admirable electrolyte (1880.), whether they be those which are related to it as an active (1879.) or as a passive (1825, &c.) body, confirm the chemical theory, and oppose that of contact. With tin and cadmium it gives an impermeable non-conducting body; with lead and bismuth it gives an impermeable conducting body; with antimony and silver it produces a permeable non-conducting body; with copper a permeable conducting body; and with zinc a soluble non-conducting body. The chemical action and its resulting current are perfectly consistent with all these variations. But try to explain them by the theory of contact, and, as far as I can perceive, that can only be done by twisting the theory about and making it still more tortuous than before (1861. 1865. 1872. 1874. 1889.); special assumptions being necessary to account for the effects which, under it, become so many special cases.

1908. *Solution of protosulphuret of potassium, or bihydrosulphuret of potassa.*—I used a solution of this kind as the electrolyte in a few cases. The results generally were in accordance with those already given, but I did not think it necessary to pursue them at length. The solution was made by passing sulphuretted hydrogen gas for twenty-fours through a strong solution of pure caustic potassa.

1909. Iron and platinum with this solution formed a circle in which the iron was first negative, then gradually became neutral, and finally acquired a positive state. The solution first acted as the yellow sulphuret in reducing the investing oxide (2049.), and then, apparently, directly on the iron, dissolving the sulphuret formed. Nickel was positive to platinum from the first, and continued so though producing only a weak current. When weak chemical action was substituted for metallic contact at *x*, fig. 2 (1831.), a powerful current passed. Copper was highly positive to iron and nickel; as also to platinum, gold, and the other metals which were unacted upon by the solution. Silver was positive to iron, nickel, and even lead; as well as to platinum, gold, &c. Lead is positive to platinum, then the current falls, but does not cease. Bismuth is also positive at first, but after a while the current almost entirely ceases, as with the yellow sulphuret of potassium (1894.).

1910. Native gray sulphuret of copper and artificial sulphuret of copper (1899.) were positive to platinum and the inactive metals: but yellow copper pyrites, yellow iron pyrites, and galena, were inactive with these metals in this solution; as before they had been with the solution of yellow or bisulphuret of potassium. This solution, as might be expected from its composition, has more of alkaline characters in it than the yellow sulphuret of potassium.

1911. Before concluding this account of results with the sulphuretted solutions, as

exciting electrolytes, I will mention the varying and beautiful phenomena which occur when copper and silver, or two pieces of copper, or two pieces of silver, form a circle with the yellow solution. If the metals be copper and silver, the copper is at first positive and the silver remains untarnished; in a short time this action ceases, and the silver becomes positive; at the same instant it begins to combine with sulphur and becomes covered with sulphuret of silver; in the course of a few moments the copper again becomes positive; and thus the action will change from side to side several times, and the current with it, according as the circumstances become in turn more favourable at one side or the other.

1912. But how can it be thought that the current first produced is due in any way to the *contact* of the sulphuret of copper formed, since its presence there becomes at last the reason why that first current diminishes, and enables the silver, which is originally the weaker in exciting force, and has no sulphuret as yet formed on it, to assume for a time the predominance, and produce a current which can overcome that excited at the copper (1911.)? What can account for these changes, but chemical action? which, as it appears to me, accounts, as far as we have yet gone, with the utmost simplicity, for *all* the effects produced, however varied the mode of action and their circumstances may be.

Royal Institution,
December 12, 1839.

III. *Experimental Researches in Electricity.—Seventeenth Series.*

By Michael Faraday, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, &c. &c.*

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§ 24. *On the source of power in the voltaic pile.*—(Continued.)

¶ iv. *The exciting chemical force affected by temperature.*
¶ v. *The exciting chemical force affected by dilution.*
¶ vi. *Differences in the order of the metallic elements of voltaic circles.*
¶ vii. *Active voltaic circles and batteries without metallic contact.*
¶ viii. *Considerations of the sufficiency of chemical action.*
¶ ix. *Thermo-electric evidence.*
¶ x. *Improbable nature of the assumed contact force.*

¶ iv. *The exciting chemical force affected by temperature.*

1913. On the view that chemical force is the origin of the electric current in the voltaic circuit, it is important that we have the power of causing by ordinary chemical means, a variation of that force within certain limits, without involving any alteration of the metallic or even the other contacts in the circuit. Such variations should produce corresponding voltaic effects, and it appeared not improbable that these differences alone might be made effective enough to produce currents without any metallic contact at all.

1914. De la Rive has shown that the increased action of a pair of metals, when put into hot fluid instead of cold, is in a great measure due to the exaltation of the chemical affinity on that metal which was acted upon*. My object was to add to the argument by using but one metal and one fluid, so that the fluid might be alike at both contacts, but to exalt the chemical force at one only of the contacts by the action of heat. If such difference produced a current with circles which either did not generate a thermo current themselves, or could not conduct that of an antimony and bismuth element, it seemed probable that the effect would prove to be a result of pure chemical force, contact doing nothing.

1915. The apparatus used was a glass tube (Plate III. fig. 7.) about five inches long

* Annales de Chimie, 1828, xxxvii. p. 242.

and 0·4 of an inch internal diameter, open at both ends, bent, and supported on a retort-stand. In this the liquid was placed, and the portion in the upper part of one limb could then easily be heated and retained so, whilst that in the other limb was cold. In the experiments I will call the left-hand side A, and the right-hand side B, taking care to make no change of these designations. C and D are the wires of metal (1881.) to be compared; they were formed into a circuit by means of the galvanometer and, often also, a SEEBECK's thermo-element of antimony and bismuth; both these, of course, caused no disturbing effect so long as the temperature of their various junctions was alike. The wires were carefully prepared (1881.), and when two of the same metal were used, they consisted of the successive portions of the same piece of wire.

1916. The precautions which are necessary for the elimination of a correct result are rather numerous, but simple in their nature.

1917. *Effect of first immersion.*—It is hardly possible to have the two wires of the same metal, even platinum, so exactly alike that they shall not produce a current in consequence of their difference; hence it is necessary to alternate the wires and repeat the experiment several times, until an undoubted result independent of such disturbing influences is obtained.

1918. *Effect of the investing fluid or substance.*—The fluid produced by the action of the liquid upon the metal exerts, as is well known, a most important influence on the production of a current. Thus when two wires of cadmium were used with the apparatus, fig. 7, (1915.) containing dilute sulphuric acid, hot on one side and cold on the other, the hot cadmium was at first positive, producing a deflection of about 10°; but in a short time this effect disappeared, and a current in the reverse direction equal to 10° or more would appear, the hot cadmium being now negative. This I refer to the quicker exhaustion of the chemical forces of the film of acid on the heated metallic surface (1003. 1036. 1037.), and the consequent final superiority of the colder side at which the action was thus necessarily more powerful (1953, &c. 1966. 2015. 2031, &c.). MARIANINI has described many cases of the effects of investing solutions, showing that if two pieces of the same metal (iron, tin, lead, zinc, &c.) be used, the one first immersed is negative to the other, and has given his views of the cause*. The precaution against this effect was not to put the metals into the acid until the proper temperature had been given to both parts of it, and then to observe the *first effect* produced, accounting that as the true indication, but repeating the experiment until the result was certain.

1919. *Effect of motion.*—This investing fluid (1918.) made it necessary to guard against the effect of successive rest and motion of the metal in the fluid. As an illustration, if two tin wires (1881.) be put into dilute nitric acid, there will probably be a little motion at the galvanometer, and then the needle will settle at 0°. If either wire be then moved, the other remaining quiet, that in motion will become positive. Again,

* Annales de Chimie, 1830, xlv. p. 40.

tin and cadmium in dilute sulphuric acid gave a strong current, the cadmium being positive, and the needle was deflected 80°. When left, the force of the current fell to 35°. If the cadmium were then moved it produced very little alteration; but if the tin were moved it produced a great change, not showing, as before, an increase of its force, but the reverse, for it became more negative, and the current force rose up again to 80°*. The precaution adopted to avoid the interference of these actions, was not only to observe the first effect of the introduced wires, but to keep them moving from the moment of the introduction.

1920. The above effect was another reason for heating the acids, &c. (1918.) before the wires were immersed; for in the experiment just described, if the cadmium side were heated to boiling, the moment the fluid was agitated on the tin side by the boiling on the cadmium side, there was more effect by far produced by the motion than the heat: for the heat at the cadmium alone did little or nothing, but the jumping of the acid over the tin made a difference in the current of 20° or 30°.

1921. *Effect of air.*—Two platinum wires were put into cold strong solution of sulphuret of potassium (1812.), fig. 7; and the galvanometer was soon at 0°. On heating and boiling the fluid on the side A (1915.) the platinum in it became negative; cooling that side, by pouring a little water over it from a jug, and heating the side B, the platinum there in turn became negative; and, though the action was irregular, the same general result occurred however the temperatures of the parts were altered. This was not due to the chemical effect of the electrolyte on the heated platinum. Nor do I believe it was a true thermo current (1933.); but if it were the latter, then the heated platinum was *negative* through the electrolyte to the cold platinum. I believe it was altogether the increased effect of the air upon the electrolyte at the heated side; and it is evident that the application of the heat, by causing currents in the fluid and also in the air, facilitates their mutual action at that place. It has been already shown, that lifting up a platinum wire in this solution, so as to expose it for a moment to the air (1827.), renders it negative when reimmersed, an effect which is in perfect accordance with the assumed action of the heated air and fluid in the present case. The interference of this effect is obviated by raising the temperature of the electrolyte quietly before the wires are immersed (1918.), and observing only the first effect.

1922. *Effect of heat.*—In certain cases where two different metals are used, there is a very remarkable effect produced on heating the negative metal. This will require

* Tin has some remarkable actions in this respect. If two tins be immersed in succession into dilute nitric acid, the one last in is positive to the other at the moment: if, both being in, one be moved, that is for the time positive to the other. But if dilute sulphuric acid be employed, the last tin is always negative: if one be taken out, cleaned, and reimmersed, it is negative: if, both being in and neutral, one be moved, it becomes negative to the other. The effects with muriatic acid are the same in kind as those with sulphuric acid, but not so strong. This effect perhaps depends upon the compound of tin first produced in the sulphuric and muriatic acids tending to acquire some other and more advanced state, either in relation to the oxygen, chlorine or acid concerned, and so adding a force to that which at the first moment, when only metallic tin and acid are present, tends to determine a current.

too much detail to be described fully here; but I will briefly point it out and illustrate it by an example or two.

1923. When two platinum wires were compared in hot and cold dilute sulphuric acid (1935.), they gave scarcely a sensible trace of any electric current. If any real effect of heat occurred, it was that the hot metal was the least degree positive. When silver and silver were compared, hot and cold, there was also no sensible effect. But when platinum and silver were compared in the same acid, different effects occurred. Both being cold, the silver in the A side fig. 7. (1915.) was positive about 4°, by the galvanometer; moving the platina on the other side B did not alter this effect, but on heating the acid and platinum there, the current became very powerful, deflecting the needle 30°, and the silver was positive. Whilst the heat continued, the effect continued; but on cooling the acid and platinum it went down to the first degree. No such effect took place at the silver; for on heating that side, instead of becoming negative, it became more positive, but only to the degree of deflecting the needle 16°. Then, *motion* of the platinum (1919.) facilitated the passing of the current and the deflection increased, but *heating* the platinum side did far more.

1924. *Silver and copper* in dilute sulphuric acid produced very little effect; the copper was positive about 1° by the galvanometer; moving the copper or the silver did nothing; heating the copper side caused no change; but on heating the silver side it became negative 20°. On cooling the silver side this effect went down, and then, either moving the silver or copper, or heating the copper side, caused very little change; but heating the silver side made it negative as before.

1925. All this resolves itself into an effect of the following kind; that where two metals are in the relation of positive and negative to each other in such an electrolyte as dilute acids (and perhaps others), heating the negative metal at its contact with the electrolyte enables the current, which tends to form, to pass with such facility, as to give a result sometimes tenfold more powerful than would occur without it. It is not displacement of the investing fluid, for motion will in these cases do nothing: it is not chemical action, for the effect occurs at that electrode where the chemical action is not active; it is not a thermo-electric phenomenon of the ordinary kind, because it depends upon a voltaic relation; i. e. the metal showing the effect must be negative to the other metal in the electrolyte; so silver heated does nothing with silver cold, though it shows a great effect with copper either hot or cold (1924.); and platinum hot is as nothing to platina cold, but much to silver either hot or cold.

1926. Whatever may be the intimate action of heat in these cases, there is no doubt that it is dependent on the current which tends to pass round the circuit. It is essential to remember that the increased effect on the galvanometer is not due to any increase in the electromotive force, but solely to the removal of obstruction to the current by an increase probably of discharge. M. DE LA RIVE has described an effect of heat, on the passage of the electric current, through dilute acid placed in the circuit, by platinum electrodes. Heat applied to the negative electrode increased the de-

flection of a galvanometer needle in the circuit, from 12° to 30° or 45°; whilst heat applied to the positive electrode caused no change*. I have not been able to obtain this nullity of effect at the positive electrode when a voltaic battery was used (1639.); but I have no doubt the present phenomena will prove to be virtually the same as those which that philosopher has described.

1927. The effect interferes frequently in the ensuing experiments when *two* metals, hot and cold, are compared with each other; and the more so as the negative metal approximates in inactivity of character to platinum or rhodium. Thus in the comparison of cold copper, with hot silver, gold, or platinum, in dilute nitric acid, this effect tends to make the copper appear more positive than it otherwise would do.

1928. *Place of the wire terminations.*—It is requisite that the *end* of the wire on the hot side should be *in* the heated fluid. Two copper wires were put into diluted solution of sulphuret of potassium, fig. 8.; that portion of the liquid extending from C to D was heated, but the part between D and E remained cold. Whilst both ends of the wires were in the cold fluid, as in the figure, there were irregular movements of the galvanometer, small in degree, leaving the B wire positive. Moving the wires about, but retaining them as in the figure, made no difference; but on raising the wire in A, so that its termination should be in the hot fluid between C and D, then it became positive and continued so. On lowering the end into the cold part, the former state recurred; on raising it into the hot part, the wire again became positive. The same is the case with two silver wires in dilute nitric acid; and though it appears very curious that the current should increase in strength as the extent of bad conductor increases, yet such is often the case under these circumstances. There can be no reason to doubt that the part of the wire which is in the hot fluid at the A side, is at all times equally positive or nearly so; but at one time the whole of the current it produces is passing through the entire circuit by the wire in B, and at another, a part, or the whole, of it is circulating to the cold end of its own wire, only by the fluid in tube A.

1929. *Cleaning the wires.*—That this should be carefully done has been already mentioned (1881.); but it is especially necessary to attend to the very extremities of the wires, for if these circular spaces, which occur in the most effective part of the circle, be left covered with the body produced on them in a preceding trial, an experimental result will often be very much deranged, or even entirely falsified.

1930. Thus the best mode of experimenting (1915.) is to heat the liquid in the limb A or B, fig. 8., first; and, having the wires well cleaned and connected, to plunge both in at once, and, retaining the *end* of the heated wire in the hot part of the fluid, to keep both wires in motion, and observe, especially, the first effects: then to take out the wires, reclean them, change them side for side and repeat the experiment, doing this so often as to obtain from the several results a decided and satisfactory conclusion.

* Bibliothèque Universelle, 1837, vii. 388.

1931. It next becomes necessary to ascertain whether any true thermo current can be produced by electrolytes and metals, which can interfere with any electro-chemical effects dependent upon the action of heat. For this purpose different combinations of electrolytes and metals not acted on chemically by them, were tried, with the following results.

1932. Platinum and a very *strong solution of potassa* gave, as the result of many experiments, the hot platinum positive across the electrolyte to the cold platinum, producing a current that could deflect the galvanometer needle about 5°, when the temperatures at the two junctures were 60° and 240°. Gold and the same solution gave a similar result. Silver and a moderately strong solution, of specific gravity 1070, like that used in the ensuing experiments (1948.) gave the hot silver positive, but now the deflection was scarcely sensible, and not more than 1°. Iron was tried in the same solution, and there was a constant current and deflection of 50° or more, but there was also chemical action (1948.).

1933. I then used *solution of the sulphuret of potassium* (1812.). As already said, hot platinum is negative in it to the cold metal (1921.); but I do not think the action was thermo-electric. Palladium with a weaker solution gave no indication of a current.

1934. Employing dilute nitric acid, consisting of one volume strong acid and fifty volumes water, platinum gave no certain indication: the hot metal was sometimes in the least degree positive, and at others an equally small degree negative. Gold in the same acid gave a scarcely sensible result; the hot metal was negative. Palladium was as gold.

1935. With dilute sulphuric acid, consisting of one by weight of oil of vitriol and eighty of water, neither platinum nor gold produced any sensible current to my galvanometer by the mere action of heat.

1936. *Muriatic acid* and platinum being conjoined, and heated as before, the hot platinum was very slightly negative in strong acid: in dilute acid there was no sensible current.

1937. *Strong nitric acid* at first seemed to give decided results. Platinum and pure strong nitric acid being heated at one of the junctions, the hot platinum became constantly negative across the electrolyte to the cold metal, the deflection being about 2°. When a yellow acid was used, the deflection was greater; and when a very orange-coloured acid was employed, the galvanometer needle stood at 70°, the hot platinum being still negative. This effect, however, is not a pure thermo current, but a peculiar result due to the presence of nitrous acid (1848.). It disappears almost entirely when a dilute acid is used (1934.); and what effect does remain indicates that the hot metal is negative to the cold.

1938. Thus the *potash solution* seems to be the fluid giving the most probable indications of a thermo current. Yet there the deflection is only 5°, though the fluid, being very strong, is a good conductor (1819.). When the fluid was diluted, and of

specific gravity 1070, like that before used (1932.), the effect was only 1°, and cannot therefore be confounded with the results I have to quote.

1939. The dilute *sulphuric* (1935.) and *nitric* acids used (1934.) gave only doubtful indications in some cases of a thermo current. On trial it was found, that the thermo current of an antimony-bismuth pair could not pass these solutions, as arranged in these and other experiments (1949. 1950.); that, therefore, if the little current obtained in the experiments be of a thermo electric nature, this combination of platinum and acid is far more powerful than the antimony-bismuth pair of SEEBECK; and yet that (with the interposed acid) it is scarcely sensible by this delicate galvanometer. Further, when there is a current, the hot metal is generally negative to the cold, and it is therefore impossible to confound these results with those to be described where the current has a contrary direction.

1940. In strong nitric acid, again, the hot metal is negative.

1941. If, after I show that heat applied to metals in acids or electrolytes which *can act on them* produces considerable currents, it be then said that though the metals which are inactive in the acids produce no thermo currents, those which, like copper, silver, &c. act chemically, may; then, I say, that such would be a mere supposition, and a supposition at variance with what we know of thermo-electricity; for amongst the solid conductors, metallic or non-metallic (1867.), there are none, I believe, which are able to produce thermo currents with some of the metals, and not with others. Further, these metals, copper, silver, &c. do not always show effects which can be mistaken or pass for thermo electric, for silver in hot dilute nitric acid is scarcely different from silver in the same acid cold (1950.); and in other cases, again, the hot metal becomes negative instead of positive (1953.).

Cases of one metal and one electrolyte; one junction being heated.

1942. The cases I have to adduce are far too numerous to be given in detail; I will therefore describe one or two, and sum up the rest as briefly as possible.

1943. *Iron in diluted sulphuret of potassium.*—The hot iron is well positive to the cold metal. The negative and cold wire continues quite clean, but from the hot iron a dark sulphuret separates, which becoming diffused through the solution discolours it. When the cold iron is taken out, washed and wiped, it leaves the cloth clean; but that which has been heated leaves a black sulphuret upon the cloth when similarly treated.

1944. *Copper and the sulphuretted solution.*—The hot copper is well positive to the cold on the first immersion, but the effect quickly falls, from the general causes already referred to (1918.).

1945. *Tin and solution of potassa.*—The hot tin is strongly and constantly positive to the cold.

1946. *Iron and dilute sulphuric acid* (1935.).—The hot iron was constantly positive

to the cold, 60° or more. *Iron and diluted nitric acid* gave even a still more striking result.

I must now enumerate merely, not that the cases to be mentioned are less decided than those already given, but to economize time.

1947. *Dilute solution of yellow sulphuret of potassium*, consisting of one volume of the strong solution (1812.), and eighteen volumes of water.—Iron, silver, and copper, with this solution, gave good results. The hot metal was positive to the cold.

1948. *Dilute solution of caustic potassa* (1932.).—Iron, copper, tin, zinc, and cadmium gave striking results in this electrolyte. The hot metal was always positive to the cold. Lead produced the same effect, but there was a momentary jerk at the galvanometer at the instant of immersion, as if the hot lead was negative at that moment. In the case of iron it was necessary to continue the application of heat, and then the formation of oxide at it could easily be observed; the alkali gradually became turbid, for the protoxide first formed was dissolved, and becoming peroxide by degrees, was deposited, and rendered the liquid dull and yellow.

1949. *Dilute sulphuric acid* (1935.).—Iron, tin, lead, and zinc, in this electrolyte, showed the power of heat to produce a current by exalting the chemical affinity, for the hot side was in each case positive.

1950. *Dilute nitric acid* is remarkable for presenting only one case of a metal hot and cold exhibiting a striking difference, and that metal is iron. With silver, copper, and zinc, the hot side is at the first moment positive to the cold, but only in the smallest degree.

1951. *Strong nitric acid.*—Hot iron is positive to cold. Both in the hot and cold acid the iron is in its peculiar state (1844. 2001.).

1952. *Dilute muriatic acid: 1 volume strong muriatic acid, and 29 volumes water.*—This acid was as remarkable for the number of cases it supplied as the dilute nitric acid was for the contrary (1950.). Iron, copper, tin, lead, zinc, and cadmium gave active circles with it, the hot metal being positive to the cold; all the results were very striking in the strength and permanency of the electric current produced.

1953. Several cases occur in which the hot metal becomes *negative* instead of positive, as above; and the principal cause of such an effect I have already adverted to (1918.). Thus with the solution of the *sulphuret of potassium* and zinc, on the first immersion of the wires into the hot and cold solution there was a pause, i. e. the galvanometer needle did not move at once, as in the former cases; afterwards a current gradually came into existence, rising in strength until the needle was deflected 70° or 80°, the hot metal being *negative* through the electrolyte to the cold metal. *Cadmium* in the same solution gave also the first pause and then a current, the hot metal being negative; but the effect was very small. Lead, hot, was negative, pro-

ducing also only a feeble current. Tin gave the same result, but the current was scarcely sensible.

1954. *In dilute sulphuric acid.*—Copper and zinc, after having produced a first positive effect at the hot metal, had that reversed, and a feeble current was produced, the hot metal being negative. Cadmium gave the same phenomena, but stronger (1918.).

1955. *In dilute nitric acid.*—Lead produced no effect at the first moment; but afterwards an electric current, gradually increasing in strength, appeared, which was able to deflect the needle 20° or more, the hot metal being negative. Cadmium gave the same results as lead. Tin gave an uncertain result: at first the hot metal appeared to be a very little negative, it then became positive, and then again the current diminished, and went down almost entirely.

1956. I cannot but view in these results of the action of heat, the strongest proofs of the dependence of the electric current in voltaic circuits on the chemical action of the substances constituting these circuits: the results perfectly accord with the known influence of heat on chemical action. On the other hand, I cannot see how the theory of contact can take cognizance of them, except by adding new assumptions to those already composing it (1874.). How, for instance, can it explain the powerful effects of iron in sulphuret of potassium, or in potassa, or in dilute nitric acid; or of tin in potassa or sulphuric acid; or of iron, copper, tin, &c. in muriatic acid; or indeed of any of the effects quoted? That they cannot be due to thermo contact has been already shown by the results with inactive metals (1931. 1941.); and to these may now be added those of the active metals, silver and copper in dilute nitric acid, for heat produces scarcely a sensible effect in these cases. It seems to me that no other cause than chemical force (a very sufficient one) remains, or is needed to account for them.

1957. If it be said that, on the theory of chemical excitement, the experiments prove either too much or not enough, that, in fact, heat ought to produce the same effect with *all* the metals that are acted on by the electrolytes used, then, I say, that that does not follow. The force and other circumstances of chemical affinity vary almost infinitely with the bodies exhibiting its action, and the added effect of heat upon the chemical affinity would, necessarily, partake of these variations. Chemical action often goes on without any current being produced; and it is well known that, in almost every voltaic circuit, the chemical force has to be considered as divided into that which is local and that which is current (1120.). Now heat frequently assists the local action much, and, sometimes, without appearing to be accompanied by any great increase in the *intensity* of chemical affinity; whilst at other times we are sure, from the chemical phenomena, that it does affect the intensity of the force. The electric current, however, is not determined by the amount of action which takes place, but

by the intensity of the affinities concerned; and so cases may easily be produced, in which that metal exerting the least amount of action is nevertheless the positive metal in a voltaic circuit; as with copper in weak nitric acid associated with other copper in strong acid (1975.), or iron or silver in the same weak acid against copper in the strong acid (1996.). Many of those instances where the hot side ultimately becomes negative, as of zinc in dilute solution of sulphuret of potassium (1953.), or cadmium and lead in dilute nitric acid (1955.), are of this nature; and yet the conditions and result are in perfect agreement with the chemical theory of voltaic excitement (1918.).

1958. The distinction between currents founded upon that difference of intensity which is due to the difference in force of the chemical action which is their exciting cause, is, I think, a necessary consequence of the chemical theory, and in 1834 I adopted that opinion* (891. 908. 916. 988.). De la Rive in 1836 gave a still more precise enunciation of such a principle†, by saying, that the intensity of currents is exactly proportional to the degree of affinity which reigns between the particles, the combination or separation of which produces the currents.

1959. I look upon the question of the origin of the power in the voltaic battery as abundantly decided by the experimental results not connected with the action of heat (1824, &c. 1878, &c.). I further view the results with heat as adding very strong confirmatory evidence to the chemical theory; and the numerous questions which arise as to the varied results produced, only tend to show how important the voltaic circuit is as a means of investigation into the nature and principles of chemical affinity (1967.). This truth has already been most strikingly illustrated by the researches of De la Rive made by means of the galvanometer, and the investigations of my friend Professor Daniell into the real nature of acid and other compound electrolytes‡.

Cases of two metals and one electrolyte; one junction being heated.

1960. Since heat produced such striking results with single metals, I thought it probable that it might be able to affect the mutual relation of the metals in some cases, and even invert their order: on making circuits with two metals and electrolytes, I found the following cases.

1961. In the solution of *sulphuret of potassium*, hot tin is well positive to cold silver: cold tin is very slightly positive to hot silver, and the silver then rapidly tarnishes.

1962. In the solution of *potassa*, cold tin is fairly positive to hot lead, but hot tin is much more positive to cold lead. Also cold cadmium is positive to hot lead, but hot cadmium is far more positive to cold lead. In these cases, therefore, there are great differences produced by heat, but the metals still keep their order.

1963. In *dilute sulphuric acid*, hot iron is *well positive* to cold tin, but hot tin is *still more positive* to cold iron. Hot iron is a little positive to cold lead, and hot lead

* Philosophical Transactions, 1834, p. 428.

† Annales de Chimie, 1836, lxi. p. 44. &c.

‡ Philosophical Transactions, 1839, p. 97.

is very positive to cold iron. These are cases of the actual inversion of order; and tin and lead may have their states reversed exactly in the same manner.

1964. *In dilute nitric acid*, tin and iron, and iron and lead may have their states reversed, whichever is the hot metal being rendered positive to the other. If, when the iron is to be plunged into the heated side (1930.) the acid is only moderately warm, it seems at first as if the tin would almost overpower the iron, so beautifully can the forces be either balanced or rendered predominant on either side at pleasure. Lead is positive to tin in both cases; but far more so when hot than when cold.

1965. These effects show beautifully that, in many cases, when two different metals are taken, either can be made positive to the other at pleasure, by acting on their chemical affinities; though the contacts of the metals with each other (supposed to be an electromotive cause,) remain *entirely unchanged*. They show the effect of heat in reversing or strengthening the natural differences of the metals, according as its action is made to oppose or combine with their natural chemical forces, and thus add further confirmation to the mass of evidence already adduced.

1966. There are here, as in the cases of one metal, some instances where the heat renders the metal more negative than it would be if cold. They occur, principally, in the solution of sulphuret of potassium. Thus, with zinc and cadmium, or zinc and tin, the coldest metal is positive. With lead and tin, the hot tin is a little positive, cold tin very positive. With lead and zinc, hot zinc is a little positive, cold zinc much more so. With silver and lead, the hot silver is a little positive to the lead, the cold silver is more, and well positive. In these cases the current is preceded by a moment of quiescence (1953.), during which the chemical action at the hot metal reduces the efficacy of the electrolyte against it more than at the cold metal, and the latter afterwards shows its advantage.

1967. Before concluding these observations on the effects of heat, and in reference to the probable utility of the voltaic circuit in investigations of the intimate nature of chemical affinity (1959.), I will describe a result which, if confirmed, may lead to very important investigations. Tin and lead were conjoined and plunged into cold dilute sulphuric acid; the tin was positive a little. The same acid was heated, and the tin and lead, having been perfectly cleaned, were reintroduced, then the lead was a little positive to the tin. So that a difference of temperature not limited to one contact, for the two electrolytic contacts were always at the same temperature, caused a difference in the relation of these metals the one to the other. Tin and iron in dilute sulphuric acid appeared to give a similar result; i. e. in the cold acid the

tin was always positive, but with hot acid the iron was sometimes positive. The effects were but small, and I had not time to enter further into the investigation.

1968. I trust it is understood that, in every case, the precautions as to very careful cleansing of the wires, the places of the ends, simultaneous immersion, observation of the first effects, &c., were attended to.

¶. v. *The exciting chemical force affected by dilution.*

1969. Another mode of affecting the chemical affinity of these elements of voltaic circuits, the metals and acids, and also applicable to the cases of such circuits, is to vary the proportion of water present. Such variation is known, by the simplest chemical experiments, to affect very importantly the resulting action, and, upon the chemical theory, it was natural to expect that it would also produce some corresponding change in the voltaic pile. The effects observed by AVOGADRO and ŒRSTED in 1823 are in accordance with such an expectation, for they found that when the same pair of metals was plunged in succession into a strong and a dilute acid, in certain cases an inversion of the current took place*. In 1828 DE LA RIVE carried these and similar cases much further, especially in voltaic combinations of copper and iron with lead†. In 1827 BECQUEREL‡ experimented with one metal, copper, plunged at its two extremities into a solution of the same substance (salt) of *different strengths*; and in 1828 DE LA RIVE§ made many such experiments with one metal and a fluid in different states of dilution, which I think of very great importance.

1970. The argument derivable from effects of this kind appeared to me so strong that I worked out the facts to some extent, and think the general results well worthy of statement. Dilution is the circumstance which most generally exalts the existing action, but how such a circumstance should increase the electromotive force of *mere contact* did not seem evident to me, without *assuming*, as before (1874.), exactly those influences at the points of contact in the various cases, which the prior results, ascertained by experiments, would require.

1971. The form of apparatus used was the bent tube already described (1915.) fig. 7. The precautions before directed with the wires, tube, &c., were here likewise needful. But there were others also requisite, consequent upon the current produced by combination of water with acid, an effect which has been described long since by BECQUEREL‖, but whose influence in the present researches requires explanation.

1972. Figs. 9 and 10 represent the two arrangements of fluids used. The part below *m* in the tubes being strong acid, and that above diluted. If the fluid was nitric acid and the platinum wires as in the figures, drawing the end of the wire D upwards above *m*, or depressing it from above *m* downwards, caused great changes at the galvanometer; but if they were preserved quiet at any place, then the electro-current ceased, or very

* Annales de Chimie, 1823, xxii. p. 361.
† Ibid. 1828, xxxvii. p. 234.
‡ Ibid. 1827, xxxv. p. 120.
§ Ibid. 1828, xxxvii. p. 240, 241.
‖ Traité de l'Électricité, ii. p. 81.

nearly so. Whenever the current existed it was from the weak to the strong acid through the liquid.

1973. When the tube was arranged, as in fig. 9, with water or dilute acid on one side only, and the wires were immersed not more than one third of an inch, the effects were greatly diminished; and more especially, if, by a little motion with a platinum wire, the acids had been mixed at *m*, so that the transition from weak to strong was gradual instead of sudden. In such cases, even when the wires were moved, horizontally, in the acid, the effect was so small as to be scarcely sensible, and not likely to be confounded with the chemical effects to be described hereafter. Still more surely to avoid such interference, an acid moderately diluted was used instead of water. The precaution was taken of emptying, washing, and rearranging the tubes with fresh acid after each experiment, lest any of the metal dissolved in one experiment should interfere with the results of the next.

1974. I occasionally used the tube with dilute acid on one side only, fig. 9, and sometimes that with dilute acid on both sides, fig. 10. I will call the first No. 1. and the second No. 2.

1975. In illustration of the general results I will describe a particular case. Employing tube No. 1. with strong and dilute nitric acid*, and two copper wires, the wire in the dilute acid was powerfully positive to the one in the strong acid at the first moment, and continued so. By using tube No. 2. the gavanometer-needle could be held stifly in either direction, simply by simultaneously raising one wire and depressing the other, so that the first should be in weak and the second in strong acid: the former was always the positive piece of metal.

1976. On repeating the experiments with the substitution of platinum, gold, or even palladium for the copper, scarcely a sensible effect was produced (1973.).

1977. *Strong and dilute nitric acid**.—The following single metals being compared with themselves in these acids, gave most powerful results of the kind just described with copper (1975.); silver, iron, lead, tin, cadmium, zinc. The metal in the weaker acid was positive to that in the stronger. Silver is very changeable, and after some time the current is often suddenly reversed, the metal in the strong acid becoming positive: this again will change back, the metal in the weaker acid returning to its positive state. With tin, cadmium, and zinc, violent action in the acid quickly supervenes and mixes all up together. Iron and lead show the alternations of state in the tube No. 2. as beautifully as copper (1975.).

1978. *Strong and dilute sulphuric acid.*—I prepared an acid of 49 by weight, strong oil of vitriol, and 9 of water, giving a sulphuric acid with two proportions of water, and arranged the tube No. 1. (1974.) with this and the strongest acid. But as this degree of dilution produced very little effect with the iron, as compared with what a

* The dilute acid consisted of three volumes of strong nitric acid and two volumes of water.

much greater dilution effected, I adopted the plan of putting strong acid into the tube, and then adding a little water at the top at one of the sides, with the precaution of stirring and cooling it previous to the experiment (1973.).

1979. With *iron*, the part of the metal in the weaker acid was powerfully positive to that in the stronger acid. With copper, the same result, as to direction of the current, was produced; but the amount of the effect was small. With silver, cadmium, and zinc, the difference was either very small or unsteady, or nothing; so that, in comparison with the former cases, the electromotive action of the strong and weak acid appeared balanced. With lead and tin, the part of the metal in the *strong* acid was *positive* to that in the weak acid; so that they present an effect the reverse of that produced by iron or copper.

1980. *Strong and dilute muriatic acid.*—I used the strongest pure muriatic acid in tube No. 1., and added water on the top of one side for the dilute extremity (1973.), stirring it a little as before. With silver, copper, lead, tin, cadmium, and zinc, the metal in the *strongest acid* was positive, and the current in most cases powerful. With iron, the end in the strongest acid was first positive: but shortly after, the weak acid side became positive and continued so. With palladium, gold, and platinum, nearly insensible effects were the results.

1981. *Strong and dilute solution of caustic potassa.*—With iron, copper, lead, tin, cadmium, and zinc, the metal in the strong solution was positive: in the case of iron slightly, in the case of copper more powerfully, deflecting the needle 30° or 38°, and in the cases of the other metals very strongly. Silver, palladium, gold, and platinum, gave the merest indications (1973.).

Thus potash and muriatic acid are, in several respects, contrasted with nitric and sulphuric acids. As respects muriatic acid, however, and perhaps even the potash, it may be admitted that, even in their strongest states, they are not fairly comparable to the very strong nitric and sulphuric acids, but rather to those acids when somewhat diluted (1985.).

1982. I know it may be said in reference to the numerous changes with strong and dilute acids, that the results are the consequence of corresponding alterations in the contact force; but this is to change about the theory with the phenomena and with chemical force (1874. 1956. 1985. 2006. 2014. 2063.); or it may be alleged that it is the contact force of the solutions produced at the metallic surfaces which, differing, causes difference of effect; but this is to put the effect before the cause in the order of *time*. If the liberty of shifting the point of efficacy from metals to fluids, or from one place to another be claimed, it is at all events quite time that some definite statement and data respecting the active points (1808.) should be given. At present it is difficult to lay hold of the contact theory by any argument derived from experiment, because of these uncertainties or variations, and it is in that respect in singular

contrast with the definite expression as to the place of action which the chemical theory supplies.

1983. All the variations which have been given are consistent with the extreme variety which chemical action under different circumstances possesses, but, as it still appears to me, are utterly incompatible with, what should be, the simplicity of mere contact action; further they admit of even greater variation, which renders the reasons for the one view and against the other, still more conclusive.

1984. Thus if a contact philosopher say that it is only the very strongest acids that can render the part of the metals in it negative, and therefore the effect does not happen with muriatic acid or potash (1980. 1981.), though it does with nitric and sulphuric acids (1977. 1978.); then, the following result is an answer to such an assumption. Iron in *dilute nitric acid*, consisting of one volume of strong acid and twenty of water, is positive to iron in strong acid, or in a mixture of one volume of strong acid with one of water, or with three, or even with five volumes of water. Silver also, in the weakest of these acids, is positive to silver in any of the other four states of it.

1985. Or if, modifying the statement upon these results, it should be said that diluting the acid at one contact *always* tends to give it a certain *proportionate* electromotive force, and therefore diluting one side more than the other will still allow this force to come into play; then, how is it that with muriatic acid and potassa the effect of dilution is the reverse of that which has been quoted in the cases with nitric acid and iron or silver? (1977. 1984.) Or if, to avoid *this difficulty*, it be assumed that each electrolyte must be considered apart, the nitric acid by itself, and the muriatic acid by itself, for that one may differ from another in the *direction* of the change induced by dilution, then how can the following results with a single acid be accounted for?

1986. I prepared four nitric acids:

A was very strong pure nitric acid;
B was one volume of A and one volume of water;
C was one volume of A and three volumes of water;
D was one volume of A and twenty volumes of water.

Experimenting with these acids and a metal, I found that copper in C acid was positive to copper in A or in D acid. Nor was it the *first* addition of water to the strong acid that brought about this curious relation, for copper in the B acid was positive to copper in the strong acid A, but negative to the copper in the weak acid D: the negative effect of the stronger nitric acid with this metal does not therefore depend upon a very high degree of concentration.

1987. Lead presents the same beautiful phenomena. In the C acid it is positive to lead either in A or D acid: in B acid it is positive to lead in the strongest, and negative to lead in the weakest acid.

1988. I prepared also three sulphuric acids:

E was strong oil of vitriol;
F one volume of E and two volumes of water;
G one volume of E and twenty volumes of water.

Lead in F was well *negative* to lead either in E or G. Copper in F was also negative to copper in E or G, but in a smaller degree. So here are two cases in which metals in an acid of a certain strength are *negative* to the same metals in the same acid, either stronger or weaker. I used platinum wires ultimately in all these cases with the same acids to check the interference of the combination of acid and water (1973.); but the results were then almost nothing, and showed that the phenomena could not be so accounted for.

1989. To render this complexity for the contact theory still more complicated, we have further variations, in which, with the same acid strong and diluted, some metals are positive in the strong acid and others in the weak. Thus, tin in the strongest sulphuric acid E (1988.) was positive to tin in the moderate or the weak acids F and G: and tin in the moderate acid F was positive to the same metal in G. Iron, on the contrary, being in the strong acid E was negative to the weaker acids F and G; and iron in the medium acid F was negative to the same metal in G.

1990. For the purpose of understanding more distinctly what the contact theory has to do here, I will illustrate the case by a diagram. Let fig. 11 represent a circle of metal and sulphuric acid. If A be an arc of iron or copper, and B C strong oil of vitriol, there will be no determinate current: or if B C be weak acid, there will be no such current: but let it be strong acid at B, and diluted at C, and an electric current will run round A C B. If the metal A be silver, it is equally indifferent with the strong and also with the weak acid, as iron has been found to be as to the production of a current; but, besides that, it is indifferent with the strong acid at B and the weak acid at C. Now if the dilution of the electrolyte at one part, as C, had so far increased the contact electromotive force there, when iron or copper was present, as to produce the current found by experiment; surely it ought (consistently with any reasonable limitations of the assumptions in the contact theory,) to have produced the same effect with silver: but there was none. Making the metal A lead or tin, the difficulty becomes far greater; for though with the strong or the weak acid alone any effect of a determinate current is nothing, yet one occurs upon dilution at C, but now dilution must be supposed to *weaken* instead of *strengthen* the contact force, for the current is in the reverse direction.

1991. Neither can these successive changes be referred to a gradual progression in the effect of dilution, dependent upon the *order of the metals*. For supposing dilution more favourable to the electromotive force of the contact of an acid and a metal, *in proportion* as the metals were in a certain order, as for instance that of their efficacy in the voltaic battery; though such an assumption might seem to account for the

gradual diminution of effect from iron to copper, and from copper to silver, one would not expect the reverse effects, or those on the other side of zero, to appear by a return back to such metals as lead and tin (1979. 1989.), but rather look for them in platinum or gold, which, however, produce no results of the kind (1976. 1988.). To increase still further this complexity, it appears, from what has been before stated, that on changing the *acids* the order must again be changed (1981.). Nay, more, that with the same acid, and merely by changing the proportion of dilution, such alteration of the order must take place (1986. 1988.).

1992. Thus it appears, as before remarked (1982.), that to apply the theory of contact electromotive force to the facts, that theory must twist and bend about with every variation of chemical action: and after all, with every variety of contact, active and inactive, in no case presents phenomena independent of the active exertion of chemical force.

1993. As the influence of dilution and concentration was so strong in affecting the relation of different parts of the same metal to an acid, making one part either positive or negative to another, I thought it probable that, by mere variation in the strength of the interposed electrolyte, the order of metals when in acids or other solutions of uniform strength, might be changed. I therefore proceeded to experiment on that point, by combining together two metals, tin and lead, through the galvanometer (1915.); arranging the electrolytic solution in tube No. 1, strong on one side and weak on the other; immersing the wires simultaneously, tin into the strong, and lead into the weak solution, and after observing the effect, re-cleaning the wires, re-arranging the fluid, and re-immersing the wires, the tin into the weak, and the lead into the strong portion. De la Rive has already stated* that inversions take place when dilute and strong sulphuric acid is used; these I could not obtain when care was taken to avoid the effect of the investing fluid (1918.): the general statement is correct, however, when applied to another acid, and I think the evidence very important to the consideration of the great question of contact or chemical action.

1994. *Two metals in strong and weak solution of potash.*—Zinc was positive to tin, cadmium, or lead, whether in the weak or strong solution. Tin was positive to cadmium, either in weak or strong alkali. Cadmium was positive to lead both ways, but most when in the strong alkali. Thus, though there were *differences in degree* dependent on the strength of the solution, there was *no inversion* of the order of the metals.

1995. *Two metals in strong and weak sulphuric acid.*—Cadmium was positive to iron and tin both ways: tin was also positive to iron, copper, and silver; and iron was positive to copper and silver, whichever side the respective metals were in. Thus none of the metals tried could be made to pass the others, and so take a different order from that which they have in acid uniform in strength. Still there were great variations in degree; thus iron in strong acid was only a little positive to silver in weak

* Annales de Chimie, 1828, xxxvii. p. 240.

acid, but iron in weak acid was very positive to silver in strong acid. Generally the metal, usually called positive, was most positive in the weak acid; but that was not the case with lead, tin, and zinc.

1996. *Two metals in strong and weak nitric acid.*—Here the degree of change produced by difference in the strength of the acid was so great, as to cause not merely difference in degree, but inversions of the order of the metals, of the most striking nature. Thus iron and silver being in tube No. 2 (1974.), whichever metal was in the weak acid was positive to the other in the strong acid. It was merely requisite to raise the one and lower the other metal to make either positive at pleasure (1975.). Copper in weak acid was positive to silver, iron, lead, or tin, in strong acid. Iron in weak acid was positive to silver, copper, lead, zinc, or tin, in strong acid. Lead in weak acid was positive to copper, silver, tin, cadmium, zinc, and iron in strong acid. Silver in weak acid was positive to iron, lead, copper, and, though slightly, even to tin, in strong acid. Tin in weak acid was positive to copper, lead, iron, zinc, and silver, and either neutral or a little positive to cadmium in strong acid. Cadmium in weak acid is very positive, as might be expected, to silver, copper, lead, iron, and tin, and, moderately so, to zinc in the strong acid. When cadmium is in the strong acid it is slightly positive to silver, copper, and iron in weak acid. Zinc in weak acid is very positive to silver, copper, lead, iron, tin, and cadmium in strong acid: when in the strong acid it is a little positive to silver and copper in weak acid.

1997. Thus wonderful changes occur amongst the metals in circuits containing this acid, merely by the effect of dilution; so that of the five metals, silver, copper, iron, lead, and tin, any one of them can be made either positive or negative to any other, with the exception of silver positive to copper. The order of these five metals only may therefore be varied above one hundred different ways in the same acid, merely by the effect of dilution.

1998. So also zinc, tin, cadmium, and lead; and likewise zinc, tin, iron, and lead, being groups each of four metals; any one of these metals may be made either positive or negative to any other metal of the same group, by dilution of this acid.

1999. But the case of variation by dilution may, as regards the opposed theories, be made even still stronger than any yet stated; for the *same metals* in the *same acid* of the *same strength at the two sides* may be made to change their order, as the chemical action of the acid on each particular metal is affected, by dilution, in a smaller or greater degree.

2000. A voltaic association of iron and silver was dipped, both metals at once, into the same strong nitric acid; for the first instant, the iron was positive; the moment after, the silver became positive, and continued so. A similar association of iron and silver was put into weak nitric acid, and the iron was immediately positive, and continued so. With iron and copper the same results were obtained.

2001. These, therefore, are *finally* cases of such an inversion (1999.); but as the iron in the strong nitric acid acquires a state the moment after its immersion, which is probably not assumed by it in the weak acid (1843. 1951. 2033.), and as the action on the iron in its *ordinary* state may be said to be, to render it positive to the silver or copper, both in the strong or weak acid, we will not endeavour to force the fact, but look to other metals.

2002. *Silver and nickel* being associated in weak nitric acid, the nickel was positive; being associated in strong nitric acid, the nickel was still positive at the first moment, but the silver was finally positive. The nickel lost its superiority through the influence of an investing film (1918.); and though the effect might easily pass unobserved, the case cannot be allowed to stand, as fulfilling the statement made (1999.).

2003. *Copper and nickel* were put into strong nitric acid; the copper was positive from the first moment. Copper and nickel being in dilute nitric acid, the nickel was slightly but clearly positive to the copper. Again, *zinc and cadmium* in strong nitric acid; the cadmium was positive strongly to the zinc; the same metals being in dilute nitric acid, the zinc was very positive to the cadmium. These I consider beautiful and unexceptionable cases (1999.).

2004. Thus the nitric acid furnishes a most wonderful variety of effects when used as the electrolytic conductor in voltaic circles; and its difference from sulphuric acid (1995.) or from potassa (1994.) in the phenomena consequent upon dilution, tend, in conjunction with many preceding facts and arguments, to show that the electromotive force in a circle is not the consequence of any power in bodies generally, belonging to them in classes rather than as individuals, and having that simplicity of character which contact force has been assumed to have, but one that has all the variations which chemical force is *known* to exhibit.

2005. The changes occurring where any one of four or five metals, differing from each other as far as silver and tin, can be made positive or negative to the others (1997. 1998.), appears to me to shut out the probability that the contact of these metals with each other can produce the smallest portion of the effect in these voltaic arrangements; and then, if not there, neither can they be effective in any other arrangements; so that what has been deduced in that respect from former experiments (1829. 1833.) is confirmed by the present.

2006. Or if the scene be shifted, and it be said that it is the *contact* of the acids or solutions which, by dilution at one side, produce these varied changes (1874. 1982. 1991. 2014. 2060.), then how *utterly unlike* such contact must be to that of the numerous class of conducting solid bodies (1809. 1867.); and where, to give the assumption any show of support, is the case of such contact (apart from chemical action) producing such currents?

2007. That it cannot be an alteration of contact force by mere dilution at one side (2006.) is also shown by making such a change, but using metals that are chemically inactive in the electrolyte employed. Thus when nitric or sulphuric acids were diluted at one side, and then the strong and the weak parts connected by platinum or gold (1976.), there was no sensible current, or only one so small as to be unimportant.

2008. A still stronger proof is afforded by the following result. I arranged the tube, fig. 9 (1972.), with strong solution of yellow sulphuret of potassium (1812.) from A to *m*, and a solution consisting of one volume of the strong solution, with six of water from *m* to B. The extremities were then connected by platinum and iron in various ways; and when the first effect of immersion was guarded against, including the first brief negative state of the iron (2049.), the effects were as follows. Platinum being in A and in B, that in A, or the strong solution, was very slightly positive, causing a permanent deflection of 2°. Iron being in A and in B, the same result was obtained. Iron being in A and platinum in B, the iron was positive about 2° to the platinum. Platinum being in A and iron in B, the platinum was now positive to the iron by about 2°. So that not only the contact of the iron and platinum passes for nothing, but the contact of strong and weak solution of this electrolyte with either iron or platinum, is ineffectual in producing a current. The current which is constant is very feeble, and evidently related to the mutual position of the strong and weak solutions, and is probably due to their gradual mixture.

2009. The results obtained by dilution of an electrolyte capable of acting on the metals employed to form with it a voltaic circuit, may in some cases depend on making the acid a better electrolyte. It would appear, and would be expected from the chemical theory, that whatever circumstance tends to make the fluid a more powerful chemical agent and a better electrolyte, (the latter being a relation purely chemical and not one of contact,) favours the production of a determinate current. Whatever the cause of the effect of dilution may be, the results still tend to show how valuable the voltaic circle will become as an investigator of the nature of chemical affinity (1959.).

¶ vi. *Differences in the order of the metallic elements of voltaic circles.*

2010. Another class of experimental arguments, bearing upon the great question of the origin of force in the voltaic battery, is supplied by a consideration of the different order in which the metals appear as electromotors when associated with different exciting electrolytes. The metals are usually arranged in a certain order; and it has been the habit to say, that a metal in the list so arranged is negative to any one above it, and positive to any one beneath it, as if (and indeed upon the conviction that) they possessed a certain direct power one with another. But in 1812 Davy showed inversions of this order in the case of iron and copper* (943.); and in 1828 De la Rive showed many inversions in different cases† (1877.); gave a strong

* Elements of Chemical Philosophy, p. 149.

† Annales de Chimie, 1828, xxxvii. 232.

contrast in the order of certain metals in strong and dilute nitric acid*; and in objecting to MARIANINI's result most clearly says, that any order must be considered in relation only to that liquid employed in the experiments from which the order is derived †.

2011. I have pursued this subject in relation to several solutions, taking the precautions before referred to (1917, &c.), and find that no such single order as that just referred to can be maintained. Thus nickel is negative to antimony and bismuth in strong nitric acid; it is positive to antimony and bismuth in dilute nitric acid; it is positive to antimony and negative to bismuth in strong muriatic acid; it is positive to antimony and bismuth in dilute sulphuric acid; it is negative to bismuth and antimony in potash; and it is very negative to bismuth and antimony, either in the colourless or the yellow solution of sulphuret of potassium.

2012. In further illustration of this subject I will take ten metals, and give their order in seven different solutions.

Dilute nitric acid.	Dilute sulphuric acid.	Muriatic acid.	Strong nitric acid.	Solution of caustic potassa.	Colourless bi-hydrosulphuret of potassium.	Yellow hydro-sulphuret of potassium.
1. Silver.	1. Silver.	3. Antimony.	5. Nickel.	1. Silver.	6. Iron.	6. Iron.
2. Copper.	2. Copper.	1. Silver.	1. Silver.	5. Nickel.	5. Nickel.	5. Nickel.
3. Antimony.	3. Antimony.	5. Nickel.	3. Antimony.	2. Copper.	4. Bismuth.	4. Bismuth.
4. Bismuth.	4. Bismuth.	4. Bismuth.	2. Copper.	6. Iron.	8. Lead.	3. Antimony.
5. Nickel.	5. Nickel.	2. Copper.	4. Bismuth.	4. Bismuth.	1. Silver.	8. Lead.
6. Iron.	6. Iron.	6. Iron.	6. Iron.	8. Lead.	3. Antimony.	1. Silver.
7. Tin.	8. Lead.	8. Lead.	7. Tin.	3. Antimony.	7. Tin.	7. Tin.
8. Lead.	7. Tin.	7. Tin.	8. Lead.	9. Cadmium.	2. Copper.	9. Cadmium.
9. Cadmium.	9. Cadmium.	9. Cadmium.	10. Zinc.	7. Tin.	10. Zinc.	2. Copper.
10. Zinc.	10. Zinc.	10. Zinc.	9. Cadmium.	10. Zinc.	9. Cadmium.	10. Zinc.

2013. The dilute nitric acid consisted of one volume strong acid and seven volumes of water; the dilute sulphuric acid, of one volume strong acid and thirteen of water; the muriatic acid, of one volume strong solution and one volume water. The strong nitric acid was pure, and of specific gravity 1·48. Both strong and weak solution of potassa gave the same order. The yellow sulphuret of potassium consisted of one volume of strong solution*(1812.) and five volumes of water. The metals are numbered in the order which they presented in the dilute acids (the negative above), for the purpose of showing, by the comparison of these numbers in the other columns, the striking departures there, from this, the most generally assumed order. Iron is included, but only in its ordinary state; its place in nitric acid being given as that which it possesses on its first immersion, not that which it afterwards acquires.

2014. The displacements appear to be most extraordinary, as extraordinary as those consequent on dilution (2005.); and thus show that there is no general ruling influence of fluid conductors, or even of acids, alkalies, &c. as distinct classes of such conductors, apart from their pure chemical relations. But how can the contact theory account for these results? To meet such facts it must be bent about in the

* Annales de Chimie, 1828, xxxvii. p. 235. † Ibid., p. 243.

most extraordinary manner, following all the contortions of the string of facts (1874. 1956. 1992. 2006. 2063.), and yet never showing a case of the production of a current by contact alone, i. e. unaccompanied by chemical action.

2015. On the other hand, how simply does the chemical theory of excitement of the current represent the facts: as far as we can yet follow them they go hand in hand. Without chemical action, no current; with the changes of chemical action, changes of current; whilst the influence of the strongest cases of *contact*, as of silver and tin (1997.) with each other, pass for nothing in the result. In further confirmation, the exciting power does not rise, but fall, by the contact of the bodies produced, as the chemical actions producing these decay or are exhausted; the consequent result being well seen in the effect of the investing fluids produced (1918. 1953. 1966.).

2016. Thus, as DE LA RIVE has said, any list of metals in their order should be constructed in reference to the exciting fluid selected. Further, a zero point should be expressed in the series; for as the electromotive power may be either at the anode or cathode (2040. 2052.), or jointly at both, that substance (if there be one) which is absolutely without any exciting action should form the zero point. The following may be given, by way of illustration, as the order of a few metals, and other substances, in relation to muriatic acid:

Peroxide of lead,
Peroxide of manganese,
Oxide of iron,
PLUMBAGO,
Rhodium,
Platinum,
Gold,
Antimony,
Silver,
Copper,
Zinc:

in which plumbago is the neutral substance; those in italics are active at the cathode, and those in Roman characters at the anode. The upper are of course negative to the lower. To make such lists as complete as they will shortly require to be, numbers expressive of the relative exciting force, counting from the zero point, should be attached to each substance.

¶ vii. *Active voltaic circles and batteries without metallic contact.*

2017. There are cases in abundance of electric currents produced by pure chemical action, but not one undoubted instance of the production of a current by pure contact. As I conceive the great question must now be settled by the weight of evidence, rather than by simple philosophic conclusions (1799.), I purpose adding a few

observations and facts to show the number of these cases, and their force. In the Eighth Series of these Researches* (April, 1834,) I gave the first experiment, that I am aware of, in which chemical action was made to produce an electric current and chemical decomposition at a distance, in a simple circuit, without any contact of metals (880, &c.). It was further shown, that when a pair of zinc and platinum plates were excited at one end by dilute nitro-sulphuric acid (880.), or solution of potash (884.), or even in some cases a solution of common salt (885.), decompositions might be produced at the other end, of solutions of iodide of potassium (900.) protochloride of tin (901.), sulphate of soda, muriatic acid, and nitrate of silver (906.); or of the following bodies in a state of fusion; nitre, chlorides of silver and lead, and iodide of lead (902. 906.); no metallic contact being allowed in any of the experiments.

2018. I will proceed to mention new cases; and first, those already referred to, where the action of a little dilute acid produced a current passing through the solution of the sulphuret of potassium (1831.), or green nitrous acid (1844.), or the solution of potassa (1854.); for here no metallic contact was allowed, and chemical action was the evident and only cause of the currents produced.

2019. The following is a table of cases of similar excitement and voltaic action, produced by chemical action without metallic contact. Each horizontal line contains the four substances forming the circuit, and they are so arranged as to give the direction of the current, which was in all cases from left to right through the bodies as they now stand. All the combinations set down were able to effect decomposition, and they are but a few of those which occurred in the course of the investigation.

2020.

Iron.	Dilute nitric acid.	Platinum.	Sulph. of potassium (1812.).	Full current.
Iron.	Dilute nitric acid.	Platinum.	Red nitric acid.	Full current.
Iron.	Dilute nitric acid.	Platinum.	Pale nitric acid, strong.	Good.
Iron.	Dilute nitric acid.	Platinum.	Green nitrous acid.	Very powerful.
Iron.	Dilute nitric acid.	Platinum.	Iodide of potassium.	Full current.
Iron.	Dilute sulphuric acid.	Platinum.	Sulphuret of potassium.	Full.
Iron.	Dilute sulphuric acid.	Platinum.	Red nitric acid.	Good.
Iron.	Muriatic acid.	Platinum.	Green nitrous acid.	Most powerful.
Iron.	Dilute muriatic acid.	Platinum.	Red nitric acid.	Good.
Iron.	Dilute muriatic acid.	Platinum.	Sulphuret of potassium.	Good.
Iron.	Solution of salt.	Platinum.	Green nitrous acid.	Most powerful.
Iron.	Common water.	Platinum.	Green nitrous acid.	Good.
Zinc.	Dilute nitric acid.	Platinum.	Iodide of potassium.	Good.
Zinc.	Muriatic acid.	Platinum.	Iodide of potassium.	Good.
Cadmium.	Dilute nitric acid.	Platinum.	Iodide of potassium.	Good.
Cadmium.	Muriatic acid.	Platinum.	Iodide of potassium.	Good.
Lead.	Dilute nitric acid.	Platinum.	Iodide of potassium.	Good.
Lead.	Muriatic acid.	Platinum.	Iodide of potassium.	Good.
Copper.	Dilute nitric acid.	Platinum.	Iodide of potassium.	
Copper.	Muriatic acid.	Platinum.	Iodide of potassium.	
Lead.	Strong sulphuric acid.	Iron.	Dilute sulphuric acid.	Strong.
Tin.	Strong sulphuric acid.	Iron.	Dilute sulphuric acid.	Strong.
Copper.	Sulphuret of potassium.	Iron.	Dilute nitric acid.	Powerful.
Copper.	Sulphuret of potassium.	Iron.	Iodide of potassium.	
Copper.	Strong nitric acid.	Iron.	Dilute nitric acid.	Very powerful.
Copper.	Strong nitric acid.	Iron.	Iodide of potassium.	
Silver.	Strong nitric acid.	Iron.	Dilute nitric acid.	Strong.
Silver.	Strong nitric acid.	Iron.	Iodide of potassium.	Good.
Silver.	Sulphuret of potassium.	Iron.	Dilute nitric acid.	Strong.
Tin.	Strong sulphuric acid.	Copper.	Dilute sulphuric acid.	

* Philosophical Transactions, 1834, p. 426.

2021. It appears to me probable that any one of the very numerous combinations which can be made out of the following Table, by taking one substance from each column and arranging them in the order in which the columns stand, would produce a current without metallic contact, and that some of these currents would be very powerful.

Rhodium Gold Platinum Palladium Silver Nickel Copper Lead Tin Zinc Cadmium	Strong nitrous acid, or strong solution of sulphuret of potassium.	Iron: Dilute nitric acid; Dilute sulphuric acid; Muriatic acid; Solution of vegetable acids; Iodide of potassium; Iodide of zinc; Solution of salt; Many metallic solutions.

2022. To these cases must be added the many in which one metal in a uniform acid gave currents when one side was heated (1942, &c.). Also those in which one metal with an acid strong and diluted gave a current (1977, &c.).

2023. In the cases where by dilution of the acid one metal can be made either positive or negative to another (1996, &c.), one half of the results should be added to the above, except that they are too strong; for instead of proving that chemical action can produce a current without contact, they go to the extent of showing a total disregard of it, and production of the current against the force of contact, as easily as with it.

2024. That it is easy to construct batteries without metallic contact was shown by Sir Humphry Davy in 1801*, when he described various effective arrangements including only one metal. At a later period Zamboni constructed a pile in which but one metal and one fluid was used†, the only difference being extent of contact at the two surfaces. The following forms, which are dependent upon the mere effect of dilution, may be added to these.

2025. Let *a b*, *a b*, *a b*, fig. 12, Plate III., represent tubes or other vessels, the parts at *a* containing strong nitric or sulphuric acid, and the parts at *b* dilute acid of the same kind; then connect these by wires, rods, or plates of one metal only, being copper, iron, silver, tin, lead, or any of those metals which become positive and negative by difference of dilution in the acid (1979, &c.). Such an arrangement will give an effective battery.

2026. If the acid used be the sulphuric, and the metal employed be iron, the current

* Philosophical Transactions, 1801, p. 397. Also Journals of the Royal Institution, 1802, p. 51; and Nicholson's Journal, 8vo, 1802, vol. i. p. 144.

† Quarterly Journal of Science, viii. 177; or Annales de Chimie, xi. 190. (1819.)

produced will be in one direction, thus ⟵, through the part figured; but if the metal be tin, the resulting current will be in the contrary direction, thus ⟶.

2027. Strong and weak solutions of potassa being employed in the tubes, then the single metals zinc, lead, copper, tin, and cadmium (1981.), will produce a similar battery.

2028. If the arrangement be as in fig. 13, in which the vessels 1, 3, 5, &c. contain strong sulphuric acid, and the vessels 2, 4, 6, &c. dilute sulphuric acid; and if the metals *a*, *a*, *a*, are tin, and *b*, *b*, *b*, are iron (1979.), a battery electric current will be produced in the direction of the arrow. If the metals be changed for each other, the acids remaining; or the acids be changed, the metals remaining; the direction of the current will be reversed.

¶ viii. *Considerations of the sufficiency of chemical action.*

2029. Thus there are no want of cases in which chemical action alone produces voltaic currents (2017.); and if we proceed to look more closely to the correspondence which ought to exist between the chemical action and the current produced, we find that the further we trace it the more exact it becomes; in illustration of which the following cases will suffice.

2030. *Chemical action does evolve electricity.*—This has been abundantly proved by Becquerel and De la Rive. Becquerel's beautiful voltaic arrangement of acid and alkali* is a most satisfactory proof that chemical action is abundantly sufficient to produce electric phenomena. A great number of the results described in the present papers prove the same statement.

2031. *Where chemical action has been, but diminishes or ceases, the electric current diminishes or ceases also.*—The cases of tin (1882. 1884.), lead (1885.), bismuth (1895.), and cadmium (1905.), in the solution of sulphuret of potassium, are excellent instances of the truth of this proposition.

2032. If a piece of grain tin be put into strong nitric acid, it will generally exert no action, in consequence of the film of oxide which is formed upon it by the heat employed in the process of breaking it up. Then two platinum wires, connected by a galvanometer, may be put into the acid, and one of them pressed against the piece of tin, yet without producing an electric current. If, whilst matters are in this position, the tin be scraped under the acid by a glass rod, or other non-conducting substance capable of breaking the surface, the acid acts on the metal newly exposed, and produces a current; but the action ceases in a moment or two from the formation of oxide of tin and an exhausted investing solution (1918.), and the current ceases with it. Each scratch upon the surface of the tin reproduces the series of phenomena.

2033. The case of iron in strong nitric acid, which acts and produces a current at the first moment (1843. 1951. 2001.), but is by that action deprived of so much of its activity, both chemical and electrical, is also a case in point.

* Annales de Chimie, 1827, xxxv. 122. Bibliothèque Universelle, 1838; xiv. 129, 171.

2034. If lead and tin be associated in muriatic acid, the lead is positive at the first moment to the tin. The tin then becomes positive, and continues so. This change I attribute to the circumstance, that the chloride of lead formed, partly invests that metal, and prevents the continuance of the action there; but the chloride of tin, being far more soluble than that of lead, passes more readily into the solution; so that action goes on there, and the metal exhibits a permanent positive state.

2035. The effect of the investing fluid already referred to in the cases of tin (1919.) and cadmium (1918.), some of the results with two metals in hot and cold acid (1966.), and those cases where metal in a heated acid became negative to the same metal in cold acid (1953, &c.), are of the same kind. The latter can be beautifully illustrated by two pieces of lead in dilute nitric acid: if left a short time, the needle stands nearly at 0°, but on heating either side, the metal there becomes negative 20° or more, and continues so as long as the heat is continued. On cooling that side and heating the other, that piece of lead which before was positive now becomes negative in turn, and so on for any number of times.

2036. *When the chemical action changes the current changes also.*—This is shown by the cases of two pieces of the same active metal in the same fluid. Thus if two pieces of silver be associated in strong muriatic acid, first the one will be positive and then the other; and the changes in the direction of the current will not be slow as if by a gradual action, but exceedingly sharp and sudden. So if silver and copper be associated in a dilute solution of sulphuret of potassium, the copper will be chemically active and positive, and the silver will remain clean; until of a sudden the copper will cease to act, the silver will become instantly covered with sulphuret, showing by that the commencement of chemical action there, and the needle of the galvanometer will jump through 180°. Two pieces of silver or of copper in solution of sulphuret of potassium produce the same effect.

2037. If metals be used which are inactive in the fluids employed, and the latter undergo no change during the time, from other circumstances, as heat, &c. (1838. 1937.), then no currents, and of course no such alterations in direction, are produced.

2038. *Where no chemical action occurs no current is produced.*—This in regard to ordinary solid conductors, is well known to be the case, as with metals and other bodies (1867.). It has also been shown to be true when fluid conductors (electrolytes) are used, in every case where they exert no chemical action, though such different substances as acid, alkalies and sulphurets have been employed (1843. 1853. 1825. 1829.). These are very striking facts.

2039. *But a current will occur the moment chemical action commences.*—This proposition may be well illustrated by the following experiment. Make an arrangement like that in fig. 14.; the two tubes being charged with the same pure, pale, strong nitric acid, the two platinum wires *p p* being connected by a galvanometer, and the wire *i*, of iron. The apparatus is only another form of the simple arrangement fig. 15., where, in imitation of a former experiment (889.), two plates of iron and platinum are placed

parallel, but separated by a drop of strong nitric acid at each extremity. Whilst in this state no current is produced in either apparatus; but if a drop of water be added at *b* fig. 15., chemical action commences, and a powerful current is produced, though without metallic or any additional contact. To observe this with the apparatus, fig. 14., a drop of water was put in at *b*. At first there was no chemical action and no electric current, though the water was there, so that contact with the water did nothing: the water and acid were moved and mixed together by means of the end of the wire *i*; in a few moments proper chemical action came on, the iron evolving nitrous gas at the place of its action, and at the same time acquiring a positive condition at that part, and producing a powerful electric current.

2040. *When the chemical action which either has or could have produced a current in one direction is reversed or undone, the current is reversed (or undone) also.*

2041. This is a principle or result which most strikingly confirms the chemical theory of voltaic excitement, and is illustrated by many important facts. VOLTA in the year 1802*, showed that crystallized *oxide of manganese* was highly negative to zinc and similar metals, giving, according to his theory, electricity to the zinc at the point of contact. BECQUEREL worked carefully at this subject in 1835†, and came to the conclusion, but reservedly expressed, that the facts were favourable to the theory of contact. In the following year DE LA RIVE examined the subject‡, and shows, to my satisfaction at least, that the peroxide is at the time undergoing chemical change and losing oxygen, a change perfectly in accordance with the direction of the current it produces.

2042. The peroxide associated with platinum in the green nitrous acid originates a current, and is negative to the platinum, at the same time giving up oxygen and converting the nitrous acid into nitric acid, a change easily shown by a common chemical experiment. In nitric acid the oxide is negative to platinum, but its negative state is much increased if a little alcohol be added to the acid, that body assisting in the reduction of the acid. When associated with platinum in solution of potash, the addition of a little alcohol singularly favours the increase of the current for the same reason. When the peroxide and platinum are associated with solution of sulphuret of potassium, the peroxide, as might have been expected, is strongly negative.

2043. In 1835 M. MUNCKE§ observed the striking power of peroxide of lead to produce phenomena like those of the peroxide of manganese, and these M. DE LA RIVE in 1836 immediately referred to corresponding chemical changes‖. M. SCHŒNBEIN does not admit this inference, and bases his view of "currents of tendency" on the phenomena presented by this body and its non-action with nitric acid¶. My own results confirm those of M. DE LA RIVE, for by direct experiment I find that the

* Annales de Chimie, 1802, xl. 224.
† Ibid. 1835, lx. 164, 171.
‡ Ibid. 1836, lxi. 40; and Bibliothèque Universelle, 1836, i. 152, 158.
§ Bibliothèque Universelle, 1836, i. 160.
‖ Ibid. 1836, i. 162, 154.
¶ Philosophical Magazine, 1838, xii. 226, 311; and Bibliothèque Universelle, 1838, xiv. 155.

peroxide is acted upon by such bodies as nitric acid. Potash and pure strong nitric acid boiled on peroxide of lead readily dissolved it, forming protonitrate of lead. A dilute nitric acid was made and divided into two portions; one was tested by a solution of sulphuretted hydrogen, and showed no signs of lead: the other was mingled with a little peroxide of lead (1822.) at common temperatures, and after an hour filtered and tested in the same manner, and found to contain plenty of lead.

2044. The peroxide of lead is negative to platinum in solutions of common salt and potash, bodies which might be supposed to exert no chemical action on it. But direct experiments show that they do exert sufficient action to produce all the effects. A circumstance in further proof that the current in the voltaic circuit formed by these bodies is chemical in its origin, is the rapid depression in the force of the current produced, after the first moment of immersion.

2045. The most powerful arrangement with peroxide of lead, platinum, and one fluid, was obtained by using a solution of the yellow sulphuret of potassium as the connecting fluid. A convenient mode of making such experiments was to form the peroxide into a fine soft paste with a little distilled water, to cover the lower extremity of a platinum plate uniformly with this paste, using a glass rod for the purpose, and making the coat only thick enough to hide the platinum well, then to dry it well, and finally, to compare that plate with a clean platinum plate in the electrolyte employed. Unless the platinum plate were perfectly covered, local electrical currents (1120.) took place which interfered with the result. In this way, the peroxide is easily shown to be negative to platinum either in the solution of the sulphuret of potassium or in nitric acid. Red-lead gave the same results in both these fluids.

2046. But using this sulphuretted solution, the same kind of proof in support of the chemical theory could be obtained from protoxides as before from the peroxides. Thus, some pure protoxide of lead, obtained from the nitrate by heat and fusion, was applied on the platinum plate (2045.), and found to be strongly negative to metallic platinum in the solution of sulphuret of potassium. White lead applied in the same manner was also found to acquire the same state. Either of these bodies when compared with platinum in dilute nitric acid was, on the contrary, very positive.

2047. The same effect is well shown by the action of oxidized iron. If a plate of iron be oxidized by heat so as to give an oxide of such aggregation and condition as to be acted on scarcely or not at all by the solution of sulphuret, then there is little or no current, such an oxide being as platinum in the solution (1840.). But if it be oxidized by exposure to air, or by being wetted and dried; or by being moistened by a little dilute nitric or sulphuric acid and then washed, first in solution of ammonia or potassa, and afterwards in distilled water and dried; or if it be moistened in solution of potassa, heated in the air, and then washed well in distilled water and dried; such iron associated with platinum and put into a solution of the sulphuret will produce a powerful current until all the oxide is reduced, the iron during the whole time being negative.

2048. A piece of rusty iron in the same solution is powerfully negative. So also is a platinum plate with a coat of protoxide, or peroxide, or native carbonate of iron on it (2045.).

2049. This result is one of those effects which has to be guarded against in the experiments formerly described (1826. 1886.). If what appears to be a clean plate of iron is put into a dilute solution of the sulphuret of potassium, it is first negative to platinum, then neutral, and at last generally feebly positive; if it be put into a strong solution, it is first negative, and then becomes neutral, continuing so. It cannot be cleansed so perfectly with sand-paper, but that when immersed it will be negative, but the more recently and well the plate has been cleansed, the shorter time does this state continue. This effect is due to the instantaneous oxidation of the surface of the iron during its momentary exposure to the atmosphere, and the after reduction of this oxide by the solution. Nor can this be considered an unnatural result to those who consider the characters of iron. Pure iron in the form of a sponge takes fire spontaneously in the air; and a plate recently cleansed if dipped into water, or breathed upon, or only exposed to the atmosphere, produces an instant smell of hydrogen. The thin film of oxide which can form during a momentary exposure is, therefore, quite enough to account for the electric current produced.

2050. As a further proof of the truth of these explanations, I placed a plate of iron under the surface of a solution of the sulphuret of potassium, and rubbed it there with a piece of wood which had been soaking for some time in the same sulphuret. The iron was then neutral or very slightly positive to platinum connected with it. Whilst in connection with the platinum it was again rubbed with the wood so as to acquire a fresh surface of contact; it did not become negative, but continued in the least degree positive, showing that the former negative current was only a temporary result of the coat of oxide which the iron had acquired in the air.

2051. Nickel appears to be subject to the same action as iron, though in a much slighter degree. All the circumstances were parallel, and the proof applied to iron (2050.) was applied to it also, with the same result.

2052. So all these phenomena with protoxides and peroxides agree in referring the current produced to chemical action; not merely by showing that the current depends upon the action, but also that the *direction* of the current depends upon the direction which the chemical affinity determines the exciting or electromotive anion to take. And it is, I think, a most striking circumstance, that these bodies, which when they can and do act chemically produce currents, have not the least power of the kind when *mere contact only* is allowed (1869.), though they are excellent conductors of electricity, and can readily carry the currents formed by other and more effectual means.

2053. With such a mass of evidence for the efficacy and sufficiency of chemical

action as that which has been given (1878. 2052.); with so many current circuits without metallic contact (2017.) and so many non-current circuits with (1867.); what reason can there be for referring the effect in the joint cases where both chemical action and contact occur, to contact, or to anything but the chemical force alone? Such a reference appears to me most unphilosophical: it is dismissing a proved and active cause to receive in its place one which is merely hypothetical.

¶ ix. *Thermo-electric evidence.*

2054. The phenomena presented by that most beautiful discovery of SEEBECK, thermo-electricity, has occasionally and, also, recently been adduced in proof of the electromotive influence of contact amongst the metals, and suchlike solid conductors* (1809. 1867.). A very brief consideration is, I think, sufficient to show how little support these phenomena give to the theory in question.

2055. If the contact of metals exert any exciting influence in the voltaic circuit, then we can hardly doubt that thermo electric currents are due to the same force; i. e. to disturbance, by local temperature, of the balanced forces of the different contacts in a metallic or similar circuit. Those who quote thermo effects as proofs of the effect of contact must, of course, admit this opinion.

2056. Admitting contact force, we may then assume that heat either increases or diminishes the electromotive force of contact. For if in fig. 16. A be antimony and B bismuth, heat applied at x causes a current to pass in the direction of the arrow; if it be assumed that bismuth in contact with antimony tends to become positive and the antimony negative, then heat diminishes the effect; but if it be supposed that the tendency of bismuth is to become negative, and of antimony positive, then heat increases the effect. How we are to decide which of these two views is the one to be adopted, does not seem to me clear; for nothing in the thermo-electric phenomena alone can settle the point by the galvanometer.

2057. If for that purpose we go to the voltaic circuit, there the situation of antimony and bismuth varies according as one or another fluid conductor is used (2012.). Antimony, being negative to bismuth with the acids, is positive to it with an alkali or sulphuret of potassium; still we find they come *nearly together* in the midst of the metallic series. In the thermo series, on the contrary, their position is at the *extremes*, being as different or as much opposed to each other as they can be. This difference was long ago pointed out by Professor CUMMING†: how is it consistent with the contact theory of the voltaic pile?

2058. Again, if silver and antimony form a thermo circle (fig. 17.), and the junction x be heated, the current there is from the silver to the antimony. If silver and bismuth form a thermo series (fig. 18.), and the junction x be heated, the current is from the bismuth to the silver; and assuming that heat increases the force of con-

* See FECHNER's words, Philosophical Magazine, 1838, xiii. p. 206.

† Annals of Philosophy, 1823, vi. 177.

tact (2056.), these results will give the direction of contact force between these metals, *antimony* ⟵ *silver*, and *bismuth* ⟶ *silver*. But in the voltaic series the current is *from the silver* to both the antimony and bismuth at their points of contact, whenever dilute sulphuric or nitric acid, or strong nitric acid, or solution of potassa (2012.) are used; so that metallic contact like that of the thermo circle, can at all events have *very little* to do here. In the yellow sulphuret of potassium the current is from both antimony and bismuth *to the silver* at their contacts, a result equally inconsistent with the thermo effect as the former. When the colourless hydrosulphuret of potassium is used to complete the voltaic circle, the current is from bismuth to silver, and from silver to antimony at their points of contact; whilst, with strong muriatic acid, precisely the reverse direction occurs, for it is from silver to bismuth, and from antimony to silver at the junctions.

2059. Again;—by the heat series copper gives a current to gold; tin and lead give currents to copper, rhodium, or gold; zinc gives one to antimony, or iron, or even plumbago; and bismuth gives one to nickel, cobalt, mercury, silver, palladium, gold, platinum, rhodium, and plumbago; at the *point of contact* between the metals,—currents which are just the reverse of those produced by the same metals, when formed into voltaic circuits and excited by the ordinary acid solutions (2012.).

2060. These, and a great number of other discrepancies, appear by a comparison, according to theory, of thermo contact and voltaic contact action, which can only be accounted for by assuming a specific effect of the contact of water, acids, alkalies, sulphurets, and other exciting electrolytes, for each metal; this assumed contact force being not only unlike thermo-metallic contact, in not possessing a balanced state in the complete circuit at uniform temperatures, but, also, having no relation to it as to the *order* of the metals employed. So bismuth and antimony, which are far apart in thermo-electric order, must have this extra character of acid contact very greatly developed in an opposite direction as to its result, to render them only a feeble voltaic combination with each other: and with respect to silver, which stands between tin and zinc thermo-electrically, not only must the same departure be required, but how great must the effect of this, its incongruous contact, be to overcome so completely as it does, and even powerfully reverse the differences which the metals (according to the contact theory) tend to produce.

2061. In further contrast with such an assumption, it must be remembered that, though the series of thermo-electric bodies is different from the usual voltaic order (2012.), it is perfectly consistent with itself, i. e. that if iron and antimony be weak with each other, and bismuth be strong with iron, it will also be strong with antimony. Also that if the electric current pass from bismuth to rhodium at the hot junction, and also from rhodium to antimony at the hot junction, it will pass far more powerfully from bismuth to antimony at the heated junction. To be at all consistent with this simple and true relation, sulphuric acid should not be strongly energetic with iron or tin and weakly so with silver, as it is in the voltaic circuit, since these

metals are not far apart in the thermo series: nor should it be nearly alike to platinum and gold voltaically, since they are far apart in the thermo series.

2062. Finally, in the thermo circuit there is that relation to heat which shows that for every portion of electric force evolved, there is a corresponding change in another force, or form of force, namely heat, able to account for it; this the united experiments of SEEBECK and PELTIER have shown. But contact force is a force which has to produce something from nothing, a result of the contact theory which can be better stated a little further on (2069. 2071. 2073.).

2063. What evidence then for mere contact excitement, derivable from the facts of thermo electricity, remains, since the power must thus be referred to the acid or other electrolyte used (2060.) and made, not only to vary uncertainly for each metal, but to vary also in direct conformity with the variation of chemical action (1874. 1956. 1992. 2006. 2014.).

2064. The contact theorist seems to consider that the advocate of the chemical theory is called upon to account for the phenomena of thermo-electricity. I cannot perceive that SEEBECK's circle has any relation to the voltaic pile, and think that the researches of BECQUEREL* are quite sufficient to authorize that conclusion.

¶ x. *Improbable nature of the assumed contact force.*

2065. I have thus given a certain body of experimental evidence and consequent conclusions, which seem to me fitted to assist in the elucidation of the disputed point, in addition to the statements and arguments of the great men who have already advanced their results and opinions in favour of the chemical theory of excitement in the voltaic pile, and against that of contact. I will conclude by adducing a further argument founded upon the, to me, unphilosophical nature of the force to which the phenomena are, by the contact theory, referred.

2066. It is assumed by the theory (1802.) that where two dissimilar metals (or rather bodies) touch, the dissimilar particles act on each other, and induce opposite states. I do not deny this, but on the contrary think, that in many cases such an effect takes place between contiguous particles; as for instance, preparatory to action in common chemical phenomena, and also preparatory to that act of chemical combination which, in the voltaic circuit, causes the current (1738. 1743.).

2067. But the contact theory assumes that these particles, which have thus by their mutual action acquired opposite electrical states, can discharge these states one to the other, and yet remain in the state they were first in, being *in every point* entirely unchanged by what has previously taken place. It assumes also that the particles, being by their mutual action rendered plus and minus, can, whilst under this inductive action, discharge to particles of like matter with themselves and so produce a current.

2068. This is in no respect consistent with known actions. If in relation to chemical

* Annales de Chimie, 1829, xli. 355. xlvi. 275.

phenomena we take two substances, as oxygen and hydrogen, we may conceive that two particles, one of each, being placed together and heat applied, they induce contrary states in their opposed surfaces, according, perhaps, to the view of Berzelius (1739.), and that these states becoming more and more exalted end at last in a mutual discharge of the forces, the particles being ultimately found combined, and unable to repeat the effect. Whilst they are under induction and before the final action comes on, they cannot spontaneously lose that state; but by removing the *cause* of the increased inductive effect, namely the heat, the effect itself can be lowered to its first condition. If the acting particles are involved in the constitution of an electrolyte, then they can produce current force (921. 924.) proportionate to the amount of chemical force consumed (868.).

2069. But the contact theory, which is obliged, according to the facts, to admit that the acting particles are not changed (1802. 2067.) (for otherwise it would be the chemical theory), is constrained to admit also, that the force which is able to make two particles assume a certain state in respect to each other, is unable to make them *retain* that state; and so it virtually denies the great principle in natural philosophy, that cause and effect are equal (2071.). If a particle of platinum by contact with a particle of zinc willingly gives of its own electricity to the zinc, because this by its presence tends to make the platinum assume a negative state, why should the particle of platinum take electricity from any other particle of platinum behind it, since that would only tend to destroy the very state which the zinc has just forced it into? Such is not the case in common induction; (and Marianini admits that the effect of contact may take place through air and measurable distances*;) for there a ball rendered negative by induction, will not take electricity from surrounding bodies, however thoroughly we may uninsulate it; and if we force electricity into it, it will, as it were, be spurned back again with a power equivalent to that of the inducing body.

2070. Or if it be supposed rather, that the zinc particle, by its inductive action, tends to make the platinum particle positive, and the latter, being in connection with the earth by other platinum particles, calls upon them for electricity, and so acquires a positive state; why should it discharge that state to the zinc, the very substance, which, making the platinum assume that condition, ought of course to be able to sustain it? Or again, if the zinc tends to make the platinum particle positive, why should not electricity go to the platinum *from the zinc*, which is as much in contact with it as its neighbouring platinum particles are? Or if the zinc particle in contact with the platinum tends to become positive, why does not electricity flow to it from the zinc particles behind, as well as from the platinum†? There is no sufficient pro-

* Memorie della Società Italiana in Modena, 1837, xxi. 232, 233, &c.

† I have spoken, for simplicity of expression, as if one metal were active and the other passive in bringing about these induced states, and not, as the theory implies, as if each were mutually subject to the other. But this makes no difference in the force of the argument; whilst an endeavour to state fully the joint changes on both sides, would rather have obscured the objections which arise, and which yet are equally strong in either view.

bable or philosophic cause assigned for the assumed action; or reason given why one or other of the consequent effects above mentioned should not take place: and, as I have again and again said, I do not know of a single fact, or case of contact current, on which, in the absence of such probable cause, the theory can rest.

2071. The contact theory assumes, in fact, that a force which is able to overcome powerful resistance, as for instance that of the conductors, good or bad, through which the current passes, and that again of the electrolytic action where bodies are decomposed by it, can arise out of nothing. That, without any change in the acting matter or the consumption of any generating force, a current can be produced which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be *a creation of power*, and is like no other force in nature. We have many processes by which the form of the power may be so changed that an apparent *conversion* of one into another takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of SEEBECK and PELTIER show the convertibility of heat and electricity; and others by ŒRSTED and myself show the convertibility of electricity and magnetism. But in no cases, not even those of the Gymnotus and Torpedo (1790.), is there a pure creation of force; a production of power without a corresponding exhaustion of something to supply it*.

2072. It should ever be remembered that the chemical theory sets out with a power, the existence of which is pre-proved, and then follows its variations, rarely assuming anything which is not supported by some corresponding simple chemical fact. The contact theory sets out with an assumption, to which it adds others as the cases require, until at last the contact force, instead of being the firm unchangeable thing at first supposed by VOLTA, is as variable as chemical force itself.

2073. Were it otherwise than it is, and were the contact theory true, then, as it appears to me, the equality of cause and effect must be denied (2069.). Then would

* (*Note*, March 29, 1840.)—I regret that I was not before aware of most important evidence for this philosophical argument, consisting of the opinion of Dr. ROGET, given in his Treatise on Galvanism in the Library of Useful Knowledge, the date of which is January 1829. Dr. ROGET is, upon the facts of the science, a supporter of the chemical theory of excitation; but the striking passage I desire now to refer to, is the following, at § 113. of the article Galvanism. Speaking of the voltaic theory of contact, he says, "Were any further reasoning necessary to overthrow it, a forcible argument might be drawn from the following consideration. If there could exist a power having the property ascribed to it by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the other known powers in nature. All the powers and sources of motion, with the operation of which we are acquainted, when producing their peculiar effects, are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect; or, in other words, a perpetual motion. But the electromotive force ascribed by VOLTA to the metals when in contact, is a force which, as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be excited with undiminished power, in the production of a never-ceasing effect. Against the truth of such a supposition, the probabilities are all but infinite."—ROGET.

the perpetual motion also be true; and it would not be at all difficult, upon the first given case of an electric current by contact alone, to produce an electro-magnetic arrangement, which, as to its principle, would go on producing mechanical effects for ever.

Royal Institution,
December 26, 1839.

NOTE.

2074. In a former series (925, &c.) I have said that I do not think any part of the electricity of the voltaic pile is due to the combination of the oxide of zinc with the sulphuric acid used, and that I agreed so far with Sir HUMPHRY DAVY in thinking that acids and alkalies did not in combining evolve electricity in large quantity when they were not parts of electrolytes.

This I would correct; for I think that BECQUEREL's pile is a perfect proof that when acid and alkali combine an electric current is produced*.

I perceive that Dr. MOHR of Coblentz appears to have shown that it is only nitric acid which amongst acids can in combining with alkalies produce an electric current†.

For myself, I had made exception of the hydracids (929.) on theoretical grounds. I had also admitted that oxyacids when in solution might in such cases produce small currents of electricity (928. and *Note.*); and JACOBI says that in BECQUEREL's improved acid and alkaline pile, it is not above a thirtieth part of the whole power which appears as current. But I now wish to say, that though in the voltaic battery, dependent for its power on the oxidizement of zinc, I do not think that the *quantity* of electricity is at all increased or affected by the combination of the oxide with the acid (933. 945.), still the latter circumstance cannot go altogether for nothing. The researches of Mr. DANIELL on the nature of compound electrolytes‡ ties together the electrolyzation of a salt and the water in which it is dissolved, in such a manner as to make it almost certain that, in the corresponding cases of the *formation* of a salt at the place of excitement in the voltaic circuit, a similar connection between the water and the salt formed must exist: and I have little doubt that the joint action of water, acids, and bases, in BECQUEREL's battery, in DANIELL's electrolyzations, and at the zinc in the ordinary active pile, are, in principle, closely connected together.

* Bibliothèque Universelle, 1838, xiv. 129. 171. Comptes rendu, i. p. 455. Annales de Chimie, 1827, xxxv. 122.

† Philosophical Magazine, 1838, xiii. p. 382; or POGGENDORF's Annalen, xlii. p. 76.

‡ Philosophical Transactions, 1839, p. 97.

IV. *Contributions to Terrestrial Magnetism.* By *Major* Edward Sabine, *R.A. V.P.R.S.*

Received March 12,—Read March 19, 1840.

§ 1. *Lines of Inclination and Intensity in the Atlantic Ocean.* § 2. *Lines of Intensity between the Cape of Good Hope and Australia.*

§ 1. *Lines of Inclination and Intensity in the Atlantic Ocean.*

THE value of observations of the magnetic inclination and intensity *made at sea*, in determining the position and direction of the magnetic lines over those large portions of the globe which are covered by the ocean, has been justly pointed out in the scientific memoranda prepared by the Royal Society for the Antarctic Expedition; and the same publication notices the precautions which are necessary in order to eliminate, in results so obtained, the disturbing influence of the ship's iron.

The observations of Lieut. Bartholomew James Sulivan, R.N., which, by his wish and with permission of the Admiralty, will form a part of this communication, afford a practical illustration of the valuable and useful information, which a series of such observations, conducted with a proper measure of skill and patience, will produce, and of the degree of certainty which may be looked for when they are thus conducted, and when the requisite precautions are taken.

At a time when increased activity has been given to magnetic observations, in the view of obtaining correct maps of the magnetic phenomena over the whole surface of the globe corresponding to the present epoch, it has appeared desirable to place in the hands of those engaged in these researches the theoretical maps of M. Gauss, accompanied by others in which the results of the most recent and authentic observations are inserted in their respective geographical positions; thus bringing under notice, as well as into comparison, the actual state both of theory and of experiment, and directing attention to those localities where observations have been most sparingly distributed, and where, consequently, they are most to be desired. This has been done, with regard to the maps of declination and intensity, in the publication of the Royal Society already referred to; and the theoretical map of the inclination has been republished from the German original in the Seventh Number of Taylor's Scientific Memoirs: but a map which should exhibit the actual observations of the latter element is yet wanting. The endeavour is made in the present communication to supply this deficiency, as far as regards that portion of the globe which is occupied by the Atlantic Ocean and its adjoining coasts. The sea observations which have

contributed to this map are the series of Lieut. Sulivan above referred to, and a series by Mr. James Dunlop of the Paramatta Observatory. The position of the lines over the surface of the land rests on above 140 determinations made on shore, within the limits of the map, between the years 1834 and 1839, of which nearly the half are now first published.

The series of Messrs. Sulivan and Dunlop include observations of the intensity made at sea, as well as of the inclination. I have made the necessary computations to reduce their results to the relative scale in general use. Previously, however, to any remarks which the consideration of these results may suggest, it will be proper to give some account of the observations from which they are derived.

Lieut. Sulivan's series was made during a voyage from Falmouth to the Falkland Islands in H.M.S. Arrow in 1838; from the Falkland Islands to Rio de Janeiro in H.M.S. Stag in 1839; and from Rio back to Falmouth in the packet-ship Opossum in the same year. The instrument employed by him was a dip circle of four inches diameter, made by Mr. Thomas Jordan of Falmouth, furnished with two needles, one for the dip and the other for the intensity, on the plan devised by Robert Were Fox, Esq., and described in the third volume of the *Annals of Electricity*, &c. The dip was in all instances observed directly, without the aid of the deflecting magnets. The values of the intensity were obtained by means of the angles of deflection from the dip, produced by the employment of constant weights applied to the axle, corrections being made for differences of temperature in the manner pointed out by Mr. Fox. The Table, Appendix A, which has been drawn up by Lieut. Sulivan, contains the particulars of the dip observations, and of the geographical positions to which they belong; the weights used on each occasion for the intensity; the angles of deflection observed, and the same corrected for temperature; the values of the intensity resulting from the experiments with each weight, and the mean values: these are given relatively to unity in Mr. Fox's garden at Falmouth, which was the base station of Lieut. Sulivan's observations, and, in the final column, in terms of the relative scale in common use, in which the force in London = 1·372, and at Falmouth 1·374*.

The circumstance of primary consideration in a series of intensity observations of this nature is the degree of steadiness with which the needle may have preserved its magnetic condition. The following observations, made in Mr. Fox's garden at Falmouth before and after the voyage, show that no notable alteration took place in this respect during its employment.

* Magnetic Survey of the British Islands, British Association Reports, 1838, p. 192.

Weights.	July 6th, 1838.	June 24th, 1839.	
	Angle of deflection.	Angle of deflection.	Corrected angle.
grains.	*Therm.* 67°.	*Therm.* 59°.	*Therm.* 67°.
0·8	29° 58′	29° 58′	30° 02′
0·9	34 16	34 16	34 20
1·0	38 47	38 33	38 37
1·1	43 33	43 39	43 43
1·2	48 32	48 52	48 56

And this conclusion is further corroborated by the following observations made with the same needle at Rio de Janeiro in September 1838, and April 1839.

Weights.	September 4th 1838.		April 4th, 1839.	
	Angle of deflection.	Corrected angle.	Angle of deflection.	Corrected angle.
grains.	*Therm.* 76°.	*Therm.* 67°.	*Therm.* 83°.	*Therm.* 67°.
0·8	53° 35′	53° 30′	53° 41′	53° 31′
0·9	65 12	65 07	65 25	65 15

When used on board ship the instrument was placed on a stand fitted with gimbals situated about the middle of the upper deck, and the same position was preserved in each of the three vessels. On a few occasions in the Arrow, the weather being very unfavourable, the observations were made in the cabin; these results are distinguished in the Table by a mark, signifying that they are entitled to less confidence than the others, and are omitted in the map.

A comparison was made on four occasions of the dip observed on board ship when in harbour, and with the same instrument taken on shore. These were as follows:

Falmouth, July 6th, 1838.

On shore	Dip 69° 12′·5
In the Arrow, ship's head W.N.W.	69 27

Rio de Janeiro, September 4th, 1838.

On shore	− 13 00·5
In the Arrow, ship's head S. by E.	− 13 56
In the Arrow, ship's head W.S.W.	− 13 37

Rio de Janeiro, April 4th, 1839.

On shore	− 13 00·5
In the Stag, ship's head N. by W.	− 13 10

Pernambuco, May 7th, 1839.

On shore	13 08·5
In the packet Opossum, head N.N.E.	13 10

In three of the comparisons the differences are inconsiderable, amounting only to a very few minutes; in the fourth it amounted in one observation nearly to a degree. It must be noticed, however, that it is more difficult to get true dip results on board a ship *at anchor, particularly where there is no tide,* than when under sail, because the ship's head cannot be kept so steadily on one point; and by the frequent variation in the direction of her head relatively to the magnetic meridian, the observed dip is rendered greater than the true; the error hence arising being greatest where the dip is least. Thus in all the four comparisons the dips observed on board are always somewhat in excess of those observed on shore; and it is probable, that the single difference which approached a degree in amount was occasioned by an accident of this nature. In the packet Opossum, in which more than the half of Lieut. Sulivan's sea observations were made, the difference is less than in either of the Queen's ships, and is much within the limits of ordinary variations in successive observations made on shore. The ship's head was always placed in these comparisons as nearly as possible on the point of the compass which corresponded with the course in that part of the voyage; and, at sea, the observations were always made with the ship's head on her course, except in one instance in the Arrow, when it was varied for the sake of experiment.

The comparison of the *intensity* results on board and on shore, in similar trials, present an even more satisfactory accord; and this consists with a remark made by Lieut. Sulivan, that by shifting the place of observation to different parts of the ship, the dip might be made to vary nearly a degree, but that in such cases the angle denoting the intensity remained nearly or wholly unaltered. The comparisons of the intensity were made only on three occasions, viz.

Rio de Janeiro, September 4th, 1838.

On shore	Intensity 0·854
In the Arrow, head S. by E.	0·855

Rio de Janeiro, April 4th, 1839.

On shore	0·854
In the Stag, head N. by W.	0·853

Pernambuco, May 7th, 1839.

On shore	0·899
In the Opossum, head N.N.E.	0·900

The four observations at Rio, viz. in the Arrow, in the Stag, and at two different periods on shore, may be regarded as identical; and the two observations at Pernambuco are not less accordant.

Lieut. Sulivan's dip results at sea are distinguished in the maps by *inclined* figures, placed on the *right* side of the mark designating the geographical position corre-

sponding to the observation. The eastern track in the northern Atlantic and the western in the southern Atlantic are those of the outward voyage in the Arrow.

In the Archives of the Royal Society is an account by Mr. James Dunlop, Astronomer of the Observatory at Paramatta, of observations on the magnetic dip and horizontal intensity made on board the merchant ship in which he took his passage from England to Australia in 1831*. These observations were repeated daily, with very few exceptions, from the middle of June to the middle of October. The dipping-needle employed was twelve inches in length, with a circle graduated to 20′. The needle was suspended in the centre of the circle by a small frame of brass, having jewelled holes for the terminations of the axle, and turning freely in azimuth, by the directive force of the needle itself, upon two fine brass points, one above and one below, working also in jewelled holes. The whole instrument was suspended by hooking inverted Y's, attached to its upper part, on two cylinders projecting from a very fine universal joint screwed to the roof of Mr. Dunlop's cabin. The universal joint was of rings, and could be turned on the centre, by the hand, in azimuth, to accommodate the plane of the instrument to that of the magnetic meridian indicated by the needle. The dip was observed by taking a mean of the extreme arcs of the small oscillations produced by the motion of the ship. The whole was inclosed in plate glass in the usual manner.

The intensity was observed by an apparatus similar to M. Hansteen's but larger, the box being about eight inches in diameter, and three inches in depth, and the tube over the centre, from which the needle was suspended by the silk fibre, was about nine inches in length. In each of the four corners of the upper side of the box was screwed a small brass pin with a hole in it, into which were hooked the ends of four copper wires which terminated in a small brass ring about $\frac{1}{4}$th of an inch in diameter over the centre of the tube, and as near the point of suspension of the needle as possible, in order that the needle and box might be suspended nearly from the same point, and also in the same line with the centre of gravity of the box. A brass wire of about $\frac{1}{12}$th of an inch in diameter was screwed about an inch into the deck, the lower end terminating in a small circular hook about the eighth of an inch in diameter, on which the box was suspended by the small ring on the vertex of the copper wires, forming a delicate universal joint.

The instruments for the dip and intensity were suspended as nearly as possible from the same part of the deck; no alteration was made in the mode of their suspension during the voyage: the trunks in the cabin contained nothing which could affect the needles, and no change was made in their disposition.

By embracing every opportunity at sea which a calm afforded of making observations with the ship's head on various points of the compass, or when alterations in the course took place in the same day, Mr. Dunlop obtained corrections to be applied to

* I was not aware of the existence of these observations when the report "on the variations of the magnetic intensity at different parts of the earth's surface" was drawn up, or they would have been included in that memoir.

the observed values of dip and intensity, according to the direction of the ship's head during the observation. His memoir contains a statement of the data on which these were founded, and the observed as well as the corrected value of every observation. The dip results have also a correction applied for the non-coincidence of the centres of gravity and of motion of the needle, the poles not being reversed; this was obtained by Mr. Dunlop by comparing the results on shore, in England and Paramatta, with those of needles in which the complete process was gone through. I have used in this paper the dips as thus corrected by Mr. Dunlop himself. The intensity results are reduced to a common temperature. The needle which was employed to give the comparative values of the intensity had sustained no sensible change in its magnetic condition for above two years previously to the voyage, as shown by observations with it at Makerstown Observatory in Scotland; nor did it undergo any in the two years subsequent to the voyage, as shown by observations at the Paramatta Observatory. That it was equally steady during the intermediate time may be inferred from the relative values which it gives for the total intensity at Makerstown and Paramatta. The times of vibration were as follows:

	s
Makerstown, March 19, 1831	822·05
Paramatta, December 7, 1831	622·90

If we take for the dip and intensity at Makerstown the values observed at the neighbouring stations of Dryburgh and Melrose*, we have the dip in March 1831, allowing for the difference of epoch, at the annual rate of a diminution of 2′·4†, 71° 50′; and the total intensity expressed by (1·372 × 1·0203 =) 1·400. At Paramatta we have the dip from the observations of Captains FitzRoy, Bethune, and Wickham, R.N., as stated in the foot note‡, 62° 51′. From these data we have the total force at Paramatta by Mr. Dunlop's needle equal to 1·665. The value determined by Capt. FitzRoy, R.N., in 1836 is 1·685. We may conclude, therefore, that Mr. Dunlop's needle suffered little or no loss of magnetism in the interval of its removal from the observatory at Makerstown to that of Paramatta.

I have availed myself of Mr. Dunlop's results for the Atlantic Maps, from latitude 6° N. and longitude 340°, where his track parted from that of Lieut. Sulivan's, to the meridian of the Cape of Good Hope. The observations are not strictly accordant in point of epoch with the other observations represented in the map; but the only quarter where this small difference of epoch ought to occasion discrepancy, is in the neighbourhood of the Cape of Good Hope, where, on account of the large amount of

* Magnetical Survey of the British Islands, British Association Reports, 1838.

† Vide the Report referred to, pp. 62—66.

‡

Captain FitzRoy (Sydney), 1836	62° 49′·4	62° 51′
Captain Bethune (Sydney), 1837	62 52·7	
Captain Wickham (Sydney), 1838 and 1839	62 51·2	

There has been little or no secular change of the dip at Sydney since the commencement of the present century. (Voyage of the Adventure and Beagle, vol. i. p. 528.)

the secular change, the south dip increases in five or six years from a half to three quarters of a degree. A discrepancy to this amount in that quarter has been allowed for in tracing the isoclinal lines in the map. The Table in the Appendix (B.) contains an abstract of the corrected dip and time of horizontal vibration corresponding to the several geographical positions, taken from Mr. Dunlop's Memoir: the final column has been computed by myself, employing as a basis the observations at Makerstown and Paramatta as above stated, with the intensity at Makerstown = 1·400.

Mr. Dunlop's results are distinguished in the maps of the Atlantic by *inclined* figures placed on the *left* side of the station mark.

The results of the dip observations *made on land* are characterized by *upright* figures; the particulars concerning them are contained in the following Table and its references. In cases where two or more observers have obtained results at the same station, the mean of their results has been inserted in the map.

North and South America.

Reference.	Station.	Latitude.	Longitude.	Dip.	Observer.	Year.
		° ′	° ′	° ′		
(a)	Montreal	+ 45 27	286 30	+ 76 19	Estcourt	1838
(c)	Oswego	+ 43 26	283 30	+ 75 11	Loomis	1839
(b)	Halifax	+ 44 39	296 23	+ 75 15	Home	1834
(a)	Halifax	+ 44 39	296 23	+ 74 45	Estcourt	1838
(c)	Utica	+ 43 09	284 45	+ 74 57	Loomis	1839
(c)	Syracuse	+ 43 00	283 45	+ 74 51	Loomis	1839
(c)	Albany	+ 42 39	286 15	+ 74 40	Bache	1834
(c)	Albany	+ 42 39	286 15	+ 74 51	Loomis	1839
(c)	Buffalo	+ 42 53	281 00	+ 74 41	Loomis	1839
(c)	Schenectady	+ 42 48	286 00	+ 74 36	Loomis	1839
(a)	Drummondville	+ 43 04	280 49	+ 74 33	Estcourt	1838
(c)	Worcester	+ 42 16	288 15	+ 74 21	Loomis	1839
(c)	Cambridge	+ 42 22	289 00	+ 74 20	Loomis	1839
(c)	Springfield	+ 42 06	287 24	+ 74 11	Bache	1834
(c)	Springfield	+ 42 06	287 24	+ 74 07	Loomis	1839
(c)	Longmeadow	+ 42 02	287 30	+ 74 05	Loomis	1839
(c)	Providence	+ 41 49	288 35	+ 74 03	Bache	1834
(c)	Providence	+ 41 49	288 35	+ 74 00	Loomis	1839
(c)	Hartford	+ 41 46	287 15	+ 73 58	Loomis	1839
(c)	Westpoint	+ 41 25	285 59	+ 73 37	Courtenay	1834
(c)	Westpoint	+ 41 25	285 59	+ 73 27	Loomis	1839
(c)	New Haven	+ 41 18	287 00	+ 73 27	Loomis	1839
(c)	New York	+ 40 43	285 59	+ 72 52	Bache	1834
(c)	New York	+ 40 43	285 59	+ 72 52	Loomis	1839
(c)	Hudson	+ 41 15	279 45	+ 72 47	Loomis	1839
(c)	Princeton	+ 40 22	285 30	+ 72 47	Loomis	1839
(c)	Beaver	+ 40 44	279 30	+ 72 40	Loomis	1839
(c)	Pittsburgh	+ 40 32	280 00	+ 72 39	Loomis	1839
(c)	Philadelphia	+ 39 57	284 48	+ 72 00	Bache	1834
(c)	Philadelphia	+ 39 57	284 48	+ 72 07	Loomis	1839
(d)	Urbana	+ 40 03	276 18	+ 71 39	Locke	1838
(d)	Springfield	+ 39 53	276 13	+ 71 29	Locke	1838
(d)	Dayton	+ 39 45	275 51	+ 71 23	Locke	1838
(e)	Washington	+ 38 53	283 00	+ 71 21	Loomis	1839
(d)	Charlottesville	+ 38 02	281 29	+ 71 09	Patterson	1834
(d)	Columbus	+ 39 55	276 57	+ 71 05	Locke	1838

North and South America.

Reference.	Station.	Latitude.	Longitude.	Dip.	Observer.	Year.
(d)	Cincinnati	+ 39° 06′	275° 33′	+ 70° 46′	Locke	1838
(b)	Bermuda	+ 32 18	295 09	+ 67 31	Home	1834 and 1837
(e)	Bermuda	+ 32 18	295 09	+ 65 56	Barnett	1835
(e)	Nassau	+ 25 05	282 39	+ 56 13	Barnett	1838 and 1839
(e)	Contoy Island	+ 21 32	273 11	+ 49 48	Barnett	1838
(f)	St. Thomas	+ 18 20	295 06	+ 49 29	Zahrtman	1834
(b)	Antigua	+ 17 03	298 10	+ 48 46	Home	1835
(b)	Alta Vela	+ 17 28	288 21	+ 47 39	Home	1835
(b)	Jamaica	+ 17 55	283 10	+ 47 04	Home	1834
(e)	Jamaica	+ 17 55	283 10	+ 47 19	Barnett	1834
(b)	Barbadoes	+ 13 05	300 23	+ 43 37	Home	1835
(e)	Cape Gracias à Dios	+ 15 00	276 42	+ 41 04	Barnett	1833
(f)	Curaçoa	+ 12 06	291 05	+ 38 39	Zahrtman	1833
(b)	Caraccas	+ 10 30	293 10	+ 37 16	Home	1836
(g)	Realejo	+ 12 28	272 48	+ 34 37	Belcher	1838
(e)	St. John's de Nicaragua	+ 10 56	276 18	+ 34 43	Barnett	1839
(b)	St. John's de Nicaragua	+ 10 58	276 17	+ 34 05	Home	1834
(b)	Demerara	+ 6 50	302 0	+ 33 57	Home	1837
(b)	Chagres	+ 9 20	280 0	+ 32 30	Home	1834
(g)	Panama	+ 8 37	280 31	+ 31 55	Belcher	1837
(g	Magnetic Island	+ 8 05	278 14	+ 31 12	Belcher	1837
(e)	Chiriqui	+ 9 00	278 05	+ 31 11	Barnett	1839
(b)	Para	− 1 26	311 30	+ 24 08	Home	1835
(g)	Cocos Island	+ 5 53	272 58	+ 23 33	Belcher	1838
(b)	Maranham	− 2 30	315 42	+ 23 31	Home	1835
(h)	Pernambuco	− 8 04	325 09	+ 13 13	FitzRoy	1836
(i)	Pernambuco	− 8 04	325 09	+ 13 08	Sulivan	1839
(g)	Puna Island	− 2 47	280 05	+ 9 08	Belcher	1838
(h)	Bahia	− 12 59	321 30	+ 5 24	FitzRoy	1832 and 1836
(k)	Bahia	− 12 59	321 30	+ 5 35	Wickham	1837
(i)	Bahia	− 12 59	321 30	+ 5 01	Sulivan	1839
(h)	Callao	− 12 04	282 52	− 7 03	FitzRoy	1835
(g)	Callao	− 12 04	282 52	− 6 14	Belcher	1838
(l)	Rio de Janeiro	− 22 55	316 51	− 12 54	Beechey	1836
(i)	Rio de Janeiro	− 22 55	316 51	− 13 00	Sulivan	1838 and 1839
(i)	St. Catherine	− 27 26	311 27	− 21 40	Beechey	1836
(i)	St. Catherine	− 27 26	311 27	− 21 07	Sulivan	1838
(i)	Monte Video	− 34 53	303 47	− 34 03	Sulivan	1838
(h)	Monte Video	− 34 53	303 47	− 34 51	FitzRoy	1833
(h)	Valparaiso	− 33 02	288 19	− 38 03	FitzRoy	1835
(l)	Valparaiso	− 33 02	288 19	− 37 05	Beechey	1836
(h)	Concepçion	− 36 42	286 50	− 43 15	FitzRoy	1835
(h)	Valdivia	− 39 53	286 31	− 46 46	FitzRoy	1835
(h)	Chiloe	− 41 51	286 04	− 48 59	FitzRoy	1835
(h)	Port Low	− 43 48	285 58	− 51 20	FitzRoy	1835
(h)	Port Desire	− 47 45	294 05	− 52 43	FitzRoy	1833
(h)	Falkland Islands	− 51 32	301 53	− 53 20	FitzRoy	1834
(i)	Falkland Islands	− 51 32	301 53	− 52 40	Sulivan	1838 and 1839
(h)	Port San Andres	− 46 35	284 25	− 54 14	FitzRoy	1834
(h)	R. Santa Cruz	− 50 07	291 36	− 55 16	FitzRoy	1834

Europe and Africa.

Reference.	Station.	Latitude.	Longitude.	Dip.	Observer.	Year.
		° ′	° ′	° ′		
(m)	London	+ 51 31	359 50	+ 69 20	Magnetic Survey	1837
(n)	Rotterdam	+ 51 55	4 26	+ 68 49	Fox	1838
(n)	Arnheim	+ 52 00	5 50	+ 68 45	Fox	1838
(o)	Brussels	+ 50 52	4 20	+ 68 31	Quetelet	1834 1839
(p)	Berlin	+ 52 30	13 24	+ 68 07	Encke	1836
(h)	Terceira	+ 38 39	332 47	+ 68 06	FitzRoy	1836
(n)	Dusseldorf	+ 51 14	6 43	+ 68 02	Fox	1838
(q)	Göttingen	+ 51 32	9 56	+ 67 50	Forbes	1837
(n)	Cologne	+ 50 57	6 46	+ 67 54	Fox	1838
(r)	Paris[a]	+ 48 52	2 21	+ 67 26	Lottin	1836
(r*)	Paris[a]	+ 48 52	2 21	+ 67 24	Arago	1835
(r)	Paris[b]	+ 48 52	2 21	+ 67 21	Duperrey	1834
(n)	Paris[c]	+ 48 52	2 21	+ 67 14	Fox	1838
(n)	Paris[d]	+ 48 52	2 21	+ 67 13	Fox	1838
(s)	Paris[a]	+ 48 52	2 21	+ 67 13	D'Abbadie	1839
(n)	Fontainbleau	+ 48 24	2 38	+ 66 59	Fox	1838
(n)	Manheim	+ 49 30	8 28	+ 66 49	Fox	1838
(n)	Baden	+ 48 45	8 17	+ 66 20	Fox	1838
(n)	Kenzigen	+ 48 10	7 47	+ 66 06	Fox	1838
(n)	Nevers	+ 47 0	3 09	+ 65 56	Fox	1838
(n)	Basle	+ 47 33	7 33	+ 65 35	Fox	1838
(n)	St. Pourcain	+ 46 19	3 17	+ 65 33	Fox	1838
(n)	Moulins	+ 46 33	3 18	+ 65 33	Fox	1838
(n)	Clermont[e]	+ 45 46	3 00	+ 65 14	Fox	1838
(n)	Clermont[f]	+ 45 47	3 05	+ 65 11	Fox	1838
(n)	Payerne	+ 46 48	6 56	+ 65 11	Fox	1838
(n)	Neweneck	+ 46 53	7 17	+ 65 10	Fox	1838
(n)	Berne	+ 46 57	7 25	+ 65 10	Fox	1838
(n)	Geneva[g]	+ 46 12	6 07	+ 64 56	Fox	1838
(n)	Geneva[h]	+ 46 12	6 07	+ 64 54	Fox	1838
(n)	Annecy	+ 45 54	6 10	+ 64 44	Fox	1838
(n)	Aix	+ 45 44	5 55	+ 64 36	Fox	1838
(n)	Grenoble	+ 45 11	5 45	+ 64 11	Fox	1838
(n)	Valence	+ 44 57	4 52	+ 64 11	Fox	1838
(t)	Milan	+ 45 28	9 09	+ 63 48	Kreil	1837
(n)	Orange	+ 44 07	4 49	+ 63 38	Fox	1838
(n)	Nismes	+ 43 50	4 20	+ 63 26	Fox	1838
(u)	Galatz	+ 45 30	28 00	+ 61 15	Ainsworth	1838
(s)	Rome	+ 41 54	12 26	+ 60 24	D'Abbadie	1839
(t)	Naples	+ 40 50	14 14	+ 58 53	Sartorius Listing	1835
(w)	Teneriffe	+ 28 27	343 45	+ 57 28	Bethune	1836
(k)	Teneriffe	+ 28 27	343 45	+ 57 47	Wickham	1837
(i)	Teneriffe	+ 28 27	343 45	+ 57 40	Sulivan	1838
(u)	Constantinople	+ 41 00	26 39	+ 56 34	Ainsworth	1838
(h)	Port Praya	+ 14 54	336 30	+ 45 46	FitzRoy	1836
(s)	Alexandria	+ 31 13	29 54	+ 43 48	D'Abbadie	1839
(x)	Egga	+ 8 45	6 45	+ 13 51	Allen	1833
(x)	Stirling	+ 7 45	7 00	+ 11 46	Allen	1833
(x)	Fernando Po	+ 3 45	8 45	+ 0 48	Allen	1833
(x)	Ascension	− 7 56	345 36	+ 1 57	Allen	1834
(h)	Ascension	− 7 56	345 36	+ 1 39	FitzRoy	1836
(x)	Ilha das Rolhas	0 0	6 30	− 7 44	Allen	1833
(h)	St. Helena	− 15 55	354 17	− 18 01	FitzRoy	1836
(h)	Cape of Good Hope	− 34 11	18 26	− 52 35	FitzRoy	1836
(k)	Cape of Good Hope	− 34 11	18 26	− 52 54	Wickham	1837
(w)	Cape of Good Hope	− 34 11	18 26	− 52 26	Bethune	1837

[a] Observatoire. [b] Depôt des Cartes. [c] Garden of the Ecole des Mines. [d] Jardin des Plantes.
[e] East side of Puy de Dôme. [f] One mile west of the city.
[g] Outside the city wall. [h] Near the observatory.

References.

(a) Magnetical observations by Lieut.-Colonel James Bucknall Estcourt, 43rd Light Infantry: MSS. in the possession of Robert Were Fox, Esq.

(b) Magnetical observations by Captain Sir Everard Home, Bart., R.N., reduced by the Rev. George Fisher, F.R.S.; Philosophical Transactions, 1838, Part II. Art. XVI.

(c) Magnetical observations in the United States. Communicated in the Proceedings of the American Philosophical Society, vol. i. No. 9.

(d) Observations of the Magnetic Dip by Messrs. Locke and Patterson, communicated by Professor Elias Loomis in Silliman's Journal, July 1838, Art. 3.

(e) Magnetical observations by Commander Edward Barnett, R.N.; MSS. in the Hydrographic Office; Appendix C.

(f) Magnetical observations by Captain Zahrtmann, of the Royal Danish Navy, reported in Dove's Repertorium, B^{d} II. page 191.

(g) Magnetical observations by Captain Edward Belcher, R.N.; MSS. in the Hydrographic Office; Appendix D.

(h) Magnetical observations made during the voyages of H.M. ships Adventure and Beagle, discussed by Major Sabine, R.A., in the Appendix to King and FitzRoy's account. The observations employed on the present occasion are those made by the officers of the Beagle from the winter of 1833–1834 to the end of the voyage.

(i) Lieut. Sulivan's observations, Appendix A.

(k) Magnetical observations by Commander John Wickham, R.N.; MSS. in the Hydrographic Office; Appendix E.

(l) Magnetical observations by Captain Frederick Beechey, R.N.; MSS. in the Hydrographic Office; Appendix F.

(m) Magnetic Survey of the British Islands, 1837; Reports of the British Association, 1838.

(n) Magnetical observations by Robert Were Fox, Esq., in manuscript; Appendix L.

(o) Mém. de l'Acad. Royale des Sciences de Bruxelles, tome xii.

(p) Astron. Jahrbuch, 1839.

(q) Gauss and Weber, Resultate, &c., 1838, p. 98.

(r) Bulletin de l'Acad. Royale des Sciences de Bruxelles, tome 6^{me}, 1^{ere} partie, p. 473.

(r*) Annuaire, 1836.

(s) Communicated in manuscript in a letter from M. D'Abbadie to Captain Beaufort, R.N.

(t) Gauss, Allgemeine Theorie, &c., page 42.

(u) Magnetical observations by William Ainsworth, Esq.; Journal of the Royal Geographical Society, vol. viii. Art. XXVI.

(w) Magnetical observations by Captain Charles Drinkwater Bethune, R.N.; MSS. in the Hydrographic Office; Appendix G.

(x) Magnetical observations by Commander William Allen, R.N.; MSS. in the Hydrographic Office; Appendix H.

In comparing the line of no dip, deduced from the observations now collected, with the same line in the map corresponding to the year 1825, taken principally from the observations of Captain DUPERREY*, the differences which appear are everywhere conformable to what is known regarding the secular changes of that element, and are within the limits of the probable effects of those changes. On the American side, the line in 1837 is *slightly* to the southward of its position in 1825. On the African side, the alteration is in the opposite direction, and is greater, corresponding to the rapidity with which the node, or point of intersection of the line of no dip and of the geographical equator, is in progress of translation to the westward. We may infer, with some degree of approximation at least, the rate at which this transference is taking place, from the observations of Captain ALLEN in December 1833 at the Ilha das Rolhas, at the southern extremity of the island of St. Thomas, compared with those which I obtained in May 1822 at Fernandilla, and at the Ilha das Cabras, both near the northern extremity of the same island. Captain ALLEN's station was distant from mine about twenty-six miles in a north and south direction, which in that part of the globe is equivalent to a difference of about twice as many minutes of dip. By my observations there was no dip at the northern extremity of St. Thomas in May 1822; and by Captain ALLEN's observations made at its southern extremity in 1833, allowing for the twenty-five miles distance apart of the two places of observation, the south dip at the northern extremity in December 1833 appears to have been about 7° S., showing a secular change of 7° in $11\frac{1}{2}$ years, or on the average 39′ in each year. Taking into account the angle which the lines of dip in that quarter make with the meridian, the intersection of the line of no dip and the geographical equator should be transferred to the westward, by a secular change of that amount, at an annual rate of half a degree. It would also follow, that in 1833 the line of no dip should be found, in or near the meridian of St. Thomas, about $3\frac{1}{2}$° to the north of the position it occupied in 1822; and that this was very nearly the fact we may infer from Captain ALLEN's observations at Fernando Po in 3° 45′ N. latitude, which gave in December 1833 a dip of 0° 48′ N.

Messrs. DUNLOP and SULIVAN's intensity results in the Atlantic Ocean are shown in Plate V., where also the isodynamic lines are drawn with the best approximation afforded by the observations hitherto obtained, including the present. Mr. DUNLOP's results are distinguished by inclined figures placed on the left of the station marks, and Lieut. SULIVAN's sea results by inclined figures placed on the right of the station marks: his land results are shown by upright figures.

In passing from the northern to the southern hemisphere, Lieut. SULIVAN found the intensity continually diminish until about the parallel of −23°, in long. 317°, from whence, in his further progress southward, it again increased. In returning from the

* Philosophical Magazine, February 1839. Represented by a dotted line in the isoclinal map in this communication.

southern to the northern hemisphere, his observation of the weakest intensity was made in $-22\frac{1}{2}°$ in the meridian of 325°. In Mr. Dunlop's passage from the North to the South Atlantic he observed the weakest intensity in $-27°$, in the more easterly meridian of 339°. The three geographical positions in which these gentlemen must have crossed the *line of least intensity* will be found, on examination, in very near accordance with my delineation of that line in the Philosophical Magazine for February 1839, which is repeated in the present map.

Lieut. Sulivan crossed the line of *no dip*, as indicated by his observations, between the latitudes $-13°\ 50'$ and $-16°\ 0'$, about the meridian of 324°, in his outward passage; and between the latitudes $-15°\ 10'$ and $-15°\ 44'$, near the same meridian, in his homeward passage. Mr. Dunlop's observations place that line in about $-13°$ latitude, in the meridian of 335° or thereabouts. These observations are also closely accordant with the representation of the line of no dip laid down in the map referred to in the Philosophical Magazine, and repeated in Plate III. They add confirmation, if any were needed, to the non-identity of the lines of no dip and of least intensity, and to their wide separation in the part of the globe under consideration. This is a marked feature in the system of the magnetic lines at the present epoch, known for some years to those who have carefully attended to the progress of experiment, but not generally recognised till very recently.

In addition to the observations of Messrs. Dunlop and Sulivan I have inserted in Plate V., in upright figures, the results of a series of intensity results obtained by Robert Were Fox, Esq., on the continent of Europe in 1838, by his own method of experimenting. The details of this series are given in the Appendix L.

§ 2. *Between the Cape of Good Hope and New South Wales.*

Mr. Dunlop's observations (Appendix B.) furnish us with a series of dip and intensity results obtained at sea between the meridian of the Cape of Good Hope and New South Wales, and between the parallels of $-35°$ and $-41°$; a part of the globe from whence no *recent* data at least have been obtained for the lines of dip, and where materials for the lines of intensity were previously wholly wanting. The form which the isodynamic lines assume in that quarter will admit of the connection being approximately traced, between Mr. Dunlop's intensity results and the positions of the lines in New South Wales as determined at several points of the coast by observations already published, or which will be now communicated. This connection is shown in Plate VI., in which the intensity results are inserted in upright figures. The materials are too few, and too widely scattered, for the lines thus traced to be regarded as more than a first approximation; but as such they may be useful. The form of the isoclinal lines in this part of the globe being less simple than that of the isodynamics, the materials are yet insufficient to admit of their being represented in any satisfactory manner; but the results themselves are inserted in the Plate in inclined figures in their respective localities.

The observations on the coast of New South Wales are contained in the following table, and the particulars of those which are now first published are given in the Appendix under the respective references.

Reference.	Station.	Latitude.	Longitude.	Dip.	Intensity.	Observer.	Year.
		° ′	° ′	° ′			
(a)	Hobart Town	−42 53	147 24	−70 35	1·817	FitzRoy	1836
(b)	Hobart Town	−42 53	147 24	−70 25	1·830	Wickham	1838
(c)	Hobart Town	−42 53	147 24	−70 31	1·810	Franklin	1837 1838
(a)	Sydney	−33 51	151 17	−62 49	1·685	FitzRoy	1836
(b)	Sydney, including Paramatta and Dunheved	−33 51	151 17	−62 51	1·682	Wickham	1838 1839
(d)	Sydney, including Paramatta and Dunheved	−33 51	151 17	−62 53		Bethune	1831
(e)	Paramatta	−33 51	151 17	−62 51	1·665	Dunlop	1831
(b)	Port George IV.	−15 20	124 40	−41 29	1·334	Wickham	1838
(b)	Point Swan	−16 21	123 03	−43 07	1·324	Wickham	1838
(b)	Port Usborne	−16 39	123 34	−43 26	1·340	Wickham	1838
(b)	Swan River	−32 03	115 41	−62 24	1·654	Wickham	1837 1838
(a)	King George's Sound	−35 02	117 56	−64 41	1·709	FitzRoy	1836
(b)	Bass's Strait	−40 28	151 35	−69 08	1·793	Wickham	1838

References.

(a) Voyage of the Adventure and Beagle, vol. i. p. 522.

(b) Observations by Commander John Wickham, R.N.; MSS. in the Hydrographic Office; Appendix E. and I.

(c) Observations made at Hobart Town at the request of the Lieut.-Governor Sir John Franklin, transmitted to Major Sabine; Appendix K.

(d) Observations by Captain Charles Drinkwater Bethune, R.N.; MSS. in the Hydrographic Office of the Admiralty; Appendix G.

(e) Observations comprised in the present communication, page 136.

APPENDIX A.

Lieut. Sulivan's observations of the Dip and Intensity made at sea between Falmouth and the Falkland Islands.

[N.B. The observations marked * were made in unfavourable weather, and in the cabin of the ship, and are not entitled to equal confidence with the others.]

Place.	Latitude.	Long. E.	Ship's head.	Dip.	Intensity. Therm.	Weight.	Angle of deflection.	Corrected angle.	Falmouth. = 1·000.	London. = 1·372.
	° ′	° ′		° ′	°	grs.	° ′	° ′		
Falmouth, Mr. Fox's Garden, July 6, 1838	50 09	354 54		69 12·5	67	0·8	29 58	29 58	1·000	1·374
						0·9	34 16	34 16	1·000	
						1·0	38 47	38 47	1·000	
						1·1	43 33	43 33	1·000	
						1·2	48 32	48 32	1·000	
H.M.S. Arrow ..	50 09	354 54	W.N.W.	69 27	.....	.....	.....	.....	.....	.....
At sea*	49 0	353 10	W.S.W.	67 18	67	1·0	40 26	40 26	0·966	1·331
						1·1	45 11	45 11	0·971	
At sea*	46 9	350 30	W.	67 49	71	1·0	40 7	40 5	0·973	1·328
						1·1	45 56	45 54	0·959	
At sea*	43 16	349 38	S.W.	65 58	75	1·0	40 48	40 43	0·961	1·323
						1·1	45 37	45 32	0·965	
At sea*	40 15	346 44	S.W.	64 32	76	1·0	40 41	40 36	0·962	1·320
						1·1	45 56	45 51	0·960	
						1·0	42 48	42 42	0·924	
At sea.....	37 28	346 4	S.W.	63 2	77	1·1	47 56	47 50	0·929	1·277
						1·2	53 19	53 13	0·935	
						1·0	43 26	43 21	0·913	
At sea.....	34 35	344 48	S.W.	61 7	75	1·1	48 37	48 32	0·920	1·265
						1·2	53 56	53 51	0·928	
						1·0	43 10	43 0	0·914	
Santa Cruz, Commandant's Garden.....	28 27	343 45		57 40	85	1·1	49 7	48 57	0·913	1·254
						1·2	55 25	55 15	0·912	
At sea.....	24 57	340 57	S.	54 15	81	1·1	51 19	51 11	0·884	1·214
			W.	55 45		1·1	51 26	51 18	0·883	
At sea.....	23 10	339 15	W.	53 26	77	1·1	52 3	51 57	0·875	1·203
At sea.....	18 20	335 30	S.W.	50 45	79	1·1	53 22	53 15	0·859	1·181
At sea.....	14 3	333 30	S. by W.	45 26	81	1·1	55 3	54 55	0·841	1·155
At sea.....	12 5	333 40	S.	42 45	80	1·1	57 7	56 59	0·822	1·129
At sea.....	8 48	334 32	S. by W.	37 7	81	1·1	60 0	59 53	0·796	1·094
At sea.....	6 32	336 23	S.E.	33 45	81	1·1	67 45	67 37	0·745	1·024
At sea.....	4 12	335 30	S.W. by S.	30 35	80	1·1	73 15	73 7	0·720	0·989
At sea.....	1 12	331 16	S.W. by S.	26 27	80	1·1	71 30	71 22	0·727	0·999
At sea.....	− 2 35	328 50	W.S.W.	21 55	82	1·1	78 0	77 51	0·705	0·969
At sea.....	− 6 20	327 20	S. by W.	15 57	82	0·8	45 45	45 36	0·699	0·956
						1·0	65 22	65 13	0·690	
At sea.....	−10 8	325 42	S. by W.	10 50	80	0·8	48 30	48 23	0·668	0·911
At sea.....	−13 50	324 41	S. by W.	1 50	78	0·8	50 45	50 40	0·646	0·881
At sea.....	−16 0	323 42	S.	− 1 25	78	0·8	50 37	50 32	0·647	0·882
At sea*	−18 35	323 0	S. by W.	− 7 40	79	0·8	50 30	50 24	0·648	0·883
Rio de Janeiro Villegagnon Is.	−22 55	316 51		−13 0·5	76	0·8	53 35	53 30	0·621	0·854
						0·9	65 12	65 7	0·621	
Rio de Janeiro H.M.S. Arrow	−22 55	316 51	S. by E.	− 13 56	77	0·8	53 30	53 24	0·622	0·855
			W.S.W.	− 13 37						
At sea.....	−24 35	315 55	S.W.	−17 55	76	0·8	52 7	52 2	0·633	0·869
At sea.....	−26 35	312 55	S.W.	−21 17	76	0·8	53 7	53 2	0·625	0·859
Santa Catherin Anhatomirim ..	−27 26	311 25		−21 7	77	0·8	50 5	49 59	0·652	0·892
						0·9	60 32	60 26	0·647	

TABLE. (Continued.)

Place.	Latitude.	Long. E.	Ship's head.	Dip.	Intensity.					
					Therm.	Weight.	Angle of deflection.	Corrected Angle.	Falmouth. = 1·000.	London. = 1·372.
	° ′	° ′		° ′	°	grs.	° ′	° ′		
At sea*	−31 44	310 40	W.S.W.	−28 22	74	0·8	45 45	45 40	0·698	0·951
						1·0	66 0	65 55	0·686	
Monte Video ..	−34 53	303 47		−34 03	72	0·8	43 24	43 22	0·727	1·004
Rat Island						1·0	58 37	58 35	0·734	
At sea.....	−38 50	303 20	S.	−40 15	60	1·0	52 45	52 49	0·786	1·080
At sea.....	−40 40	304 40	S.S.W.	−43 15	61	1·0	50 52	50 55	0·807	1·109
At sea.....	−42 6	302 50	S.	−45 45	62	1·0	48 7	48 10	0·841	1·155
At sea*	−46 0	299 50	S.S.W.	−50 15	61	1·0	40 52	40 55	0·956	1·313
Berkley Sound,						1·0	41 3	41 14	0·950	
Falkland Islands,	.....	.....		−52 40	51	1·1	46 22	46 33	0·949	
Oct. 18, 1838.						1·2	52 51	52 2	0·938	
						0·8	32 27	32 34	0·928	
Dec. 17, 1838.	−51 32	301 53		−52 38	56	0·9	36 41	36 48	0·940	1·295
						1·0	41 26	41 33	0·944	
						1·1	46 41	46 48	0·947	
						0·9	36 56	36 58	0·936	
Feb. 16, 1839.	.....	.....		−52 43	63	1·0	41 19	41 21	0·948	
						1·1	46 55	46 57	0·943	
						1·2	52 59	53 1	0·938	
H.M.S. Stag ..	−49 40	305 13	N. by E.	−49 45	66	1·0	44 0	44 1	0·902	1·236
At sea.....						1·1	50 7	50 8	0·898	
At sea.....	−46 53	309 6	N.N.E.	−45 54	59	1·0	48 37	48 42	0·834	1·150
						1·1	55 0	55 5	0·840	
At sea.....	−44 4	312 1	N.N.E.	−43 15	63	1·0	50 45	50 47	0·809	1·117
						1·1	57 15	57 17	0·818	
At sea.....	−42 2	314 49	N.N.E.	−40 45	63	1·0	56 7	56 9	0·754	1·039
						1·1	65 22	65 24	0·758	
At sea.....	−40 50	315 40	N. by E.	−37 45	63	1·0	59 0	59 2	0·730	1·005
						1·1	70 7	70 9	0·732	
At sea.....	−38 48	316 8	N. by E.	−36 15	62	0·9	52 0	52 3	0·714	0·981
						1·0	61 15	61 18	0·714	
At sea.....	−36 10	317 5	N. by E.	−34 30	67	0·9	55 22	55 22	0·684	0·944
						1·0	65 15	65 15	0·690	
At sea.....	−29 47	315 50	N.N.W.	−24 0	72	0·8	51 37	51 35	0·638	0·880
						0·9	61 22	61 20	0·641	
At sea.....	−24 40	317 35	N.	−16 57	74	0·8	54 52	54 48	0·612	0·847
						0·9	65 15	65 11	0·620	
Rio de Jan., Villegagnon Well, April 4, 1839.	−22 55	316 51		−13 05	83	0·8	53 41	53 31	0·621	0·854
						0·9	65 25	65 15	0·620	
Rio de Janeiro,	−22 55	316 51	N. by W.	−13 10	76	0·8	53 38	53 33	0·621	0·853
H.M.S. Stag ..						0·9	65 30	65 25	0·619	
Opossum Packet	−23 09	317 5	S.E.	−13 55	83	0·8	54 17	54 7	0·617	0·850
At sea.....						0·9	65 37	65 27	0·619	
At sea.....	−23 27	321 50	N.E. by E.	−12 52	82	0·8	57 0	56 51	0·598	0·824
						0·9	69 30	69 21	0·602	
At sea.....	−23 5	323 39	N.E. by E.	−13 0	81	0·8	57 59	57 51	0·590	0·813
						0·9	71 22	71 14	0·594	
At sea.....	−22 21	325 34	N.E. by E.	−12 37	80	0·8	59 45	59 38	0·579	0.800
						0·9	74 11	74 4	0·585	
At sea.....	−21 40	325 55	N.	−10 55	81	0·8	55 56	55 48	0·604	0·829
						0·9	69 11	69 3	0·603	
At sea.....	−19 56	325 5	N. by W.	− 7 7	82	0·8	54 56	54 47	0·612	0·841
						0·9	67 4	66 55	0·612	
At sea.....	−17 52	324 36	N.	− 3 37	82	0·8	53 52	53 43	0·620	0·854
						0·9	65 3	64 54	0·622	

TABLE. (Continued.)

Place.	Latitude.	Long. E.	Ship's head.	Dip.	Intensity.						
					Therm.	Weight.	Angle of deflection.	Corrected angle.	Falmouth. = 1·000.	London. = 1·372.
	° ′	° ′		° ′	°	grs.	° ′	° ′		
At sea	−15 44	323 58	N.	− 0 22	81	0·8	52 37	52 29	0·630	0·864
						0·9	63 41	63 33	0·629	
At sea	−15 10	323 26	N.N.W.	1 37	82	0·8	52 37	52 28	0·630	0·867
						0·9	63 7	62 58	0·632	
At sea	−13 36	322 20	N.N.W.	4 22	82	0·8	51 18	51 8	0·642	0·883
						0·9	61 3	60 53	0·644	
Bahia Arsenal	−12 59	321 30		5 1	84	0·8	53 18	53 8	0·624	0·859
						0·9	64 24	64 14	0·625	
Pernambuco Arsenal	− 8 04	325 9		13 8½	85	0·8	50 45	50 34	0·651	0·900
						0·9	59 9	58 58	0·657	
						1·0	73 22	73 11	0·654	
Pernambuco, Packet Opossum	− 8 04	325 9	N.N.E.	13 10	84	0·8	49 46	49 36	0·656	0·900
						0·9	59 31	59 21	0·654	
At sea	− 6 23	325 20	N. by E.	15 57	84	0·8	47 0	46 50	0·682	0·939
						0·9	55 45	55 35	0·685	
At sea	− 2 54	325 03	N. by E.	22 32	84	0·8	45 7	44 57	0·707	0·974
						0·9	52 37	52 27	0·710	
At sea	− 0 27	324 44	N. by E.	27 32	83	0·8	42 37	42 27	0·740	1·018
At sea	1 10	324 39	N. by W.	30 32	83	0·9	47 21	47 11	0·762	1·051
						1·0	55 24	55 14	0·767	
At sea	2 7	324 19	N.W.	32 2	85	0·9	47 21	47 10	0·768	1·051
						1·0	55 20	55 9	0·763	
At sea	5 10	322 53	N.W. by N.	36 22	80	0·9	43 54	43 47	0·814	1·116
						1·0	50 40	50 33	0·811	
At sea	7 22	320 53	N.N.W.	39 32	80	0·9	41 14	41 7	0·856	1·172
						1·0	47 31	47 24	0·851	
At sea	10 7	319 51	N. by W.	42 27	79	0·9	39 37	39 30	0·885	1·214
						1·0	45 22	45 15	0·882	
At sea	12 50	318 28	N.N.W.	47 5	79	0·9	38 25	38 18	0·908	1·248
						1·0	43 41	43 34	0·908	
At sea	15 40	317 36	N. ¼ W.	50 7	79	0·9	36 46	36 39	0·943	1·299
						1·0	41 30	41 23	0·947	
At sea	18 28	317 7	N. ¼ E.	52 57	79	1·0	41 1	40 54	0·957	1·314
						1·1	46 18	46 11	0·955	
At sea	21 32	316 43	N.N.E.	56 50	79	1·0	39 16	39 9	0·992	1·366
						1·1	43 50	43 43	0·996	
At sea	23 49	316 40	N.N.E.	58 47	79	1·0	37 56	37 49	1·021	1·403
						1·1	42 27	42 20	1·023	
At sea	24 43	316 55	N.E. by N.	59 47	80	1·0	37 53	37 45	1·023	1·410
						1·1	42 9	42 1	1·029	
At sea	25 48	317 19	N.E. by N.	60 50	79	1·0	37 12	37 5	1·039	1·429
						1·1	41 35	41 28	1·041	
At sea	27 10	317 49	N.E. by N.	62 2	79	1·0	37 0	36 53	1·044	1·436
						1·1	41 15	41 8	1·047	
At sea	27 51	318 27	N.E. by N.	62 27	79	1·0	36 35	36 28	1·054	1·449
						1·1	40 55	40 48	1·055	
At sea	28 36	317 30	N.W. by W.	62 52	78	1·0	36 26	36 19	1·058	1·454
						1·1	40 45	40 38	1·058	
At sea	30 0	318 5	N.E.	64 5	79	1·0	36 9	36 2	1·065	1·464
						1·1	40 25	40 18	1·065	
At sea	31 57	319 0	N.E.	64 55	78	1·0	36 11	36 4	1·064	1·470
						1·1	39 55	39 48	1·076	
At sea	34 27	320 54	E.N.E.	67 50	69	1·0	35 17	35 16	1·085	1·490
						1·1	39 23	39 22	1·086	
At sea	35 9	321 58	N.N.E.	67 35	68	1·0	34 58	34 57	1·093	1·499
						1·1	39 13	39 12	1·090	

TABLE. (Continued.)

Place.	Latitude.	Long. E.	Ship's head.	Dip.	Intensity.					
					Therm.	Weight.	Angle of deflection.	Corrected angle.	Falmouth. = 1·000.	London. = 1·372.
	° ′	° ′		° ′	°	grs.	° ′	° ′		
At sea	36 53	322 30	N.E. by E.	68 15	66	1·0	35 0	35 1	1·092	1·498
						1·1	39 19	39 20	1·087	
At sea	38 21	323 40	E.N.E.	69 42	67	1·0	35 15	35 15	1·085	1·490
						1·1	39 22	39 22	1·086	
At sea	39 44	325 54	E.	69 25	67	1·0	35 55	35 55	1·068	1·473
						1·1	39 52	39 52	1·075	
At sea	40 49	328 22	E.	70 2	67	1·0	36 7	36 7	1·063	1·467
						1·1	40 2	40 2	1·071	
At sea	41 56	330 40	E.	69 52	67	1·0	36 46	36 46	1·047	1·443
						1·1	40 55	40 55	1·052	
At sea	42 42	333 8	E.	69 50	67	1·0	36 57	36 57	1·042	1·431
						1·1	41 30	41 30	1·040	
At sea	43 32	336 5	E. by S.	69 47	64	1·0	37 10	37 12	1·036	1·425
						1·1	41 33	41 35	1·038	
At sea	45 27	342 53	E. ¼ N.	69 30	61	1·0	37 32	37 36	1·027	1·410
						1·1	42 10	42 14	1·025	
At sea	47 7	346 52	E. ¼ N.	68 52	62	1·0	38 15	38 18	1·011	1·392
						1·1	42 40	42 43	1·015	
Falmouth, Mr. Fox's Garden, June 24, 1839.	50 09	354 54		69 18	59	0·8	29 58	30 02	0·998	1·374
						0·9	34 16	34 20	0·998	
						1·0	38 33	38 37	1·004	
						1·1	43 39	43 43	0·997	
						1·2	48 52	48 56	0·994	

B.

Mr. Dunlop's Observations of the Dip and Intensity, made at sea in a voyage from England to Australia.

Date.	Latitude.	Longitude. E.	Dip.	Time of horizontal vibration.	Intensity. London = 1·372.	Date.	Latitude.	Longitude. E.	Dip.	Time of horizontal vibration.	Intensity. London = 1·372.
1831.	° ′	° ′	° ′	s		1831.	° ′	° ′	° ′	s	
July 23.	S. 5 37	341 3	28 27	576·0	1·01	Aug. 26.	S. 35 31	344 15	S. 35 44	642·4	0·88
24.	5 23	342 35	26 13	576·6	1·00	27.	36 36	347 24	38 02	653·8	0·88
25.	4 57	344 16	24 34	570·6	1·00	28.	37 12	350 24	39 16	652·0	0·90
26.	4 48	346 3	23 21	568·8	1·00	29.	37 37	353 36	40 50	654·4	0·91
27.	4 14	345 31	22 38	570·7	0·98	30.	37 40	355 51	41 51	658·1	0·92
28.	3 52	347 2	20 51	Not obs.		31.	38 44	0 16	43 44	676·2	0·91
28.	3 36	347 53	20 5	569·5	0·97	Sept. 1.	39 03	3 16	44 48	678·2	0·91
29.	3 18	349 16	19 37	568·9	0·97	2.	39 45	4 40	46 24	680·0	0·92
30.	2 37	347 40	19 44	565·0	0·99	3.	39 10	6 20	46 55	686·1	0·93
30.	2 27	347 20	19 14	Not obs.		4.	39 00	10 12	48 31	692·0	0·93
31.	1 53	346 20	18 35	569·6	0·96	5.	39 07	14 0	49 55	689·4	0·96
Aug. 1.	N. 0 53	344 36	17 22	569·6	0·95	6.	39 22	17 24	50 59	691·5	0·98
2.	1 52	342 31	14 40	571·0	0·94	7.	39 06	20 00	52 03	703·1	0·97
3.	4 3	341 37	11 28	568·1	0·93	8.	39 00	23 00	54 01	704·4	1·01
4.	6 2	341 02	8 24	573·2	0·91	9.	38 04	25 00	55 10	697·7	1·06
5.	8 10	339 50	5 12	577·7	0·89	11.	39 16	30 27	58 11	712·4	1·10
6.	9 45	337 53	3 09	574·9	0·89	12.	39 26	33 30	59 05	716·0	1·12
7.	10 12	337 42	1 38	581·5	0·87	13.	39 16	35 37	60 01	731·4	1·10
8.	12 51	336 10	0 35	577·7	0·88	14.	37 30	37 27	58 11	720·1	1·08
9.	14 31	334 30	1 05	583·4	0·87	15.	35 20	39 07	58 33	712·7	1·11
10.	16 12	333 10	4 02	581·0	0·88	16.	35 10	40 50	59 54	718·6	1·14
11.	18 09	332 48	7 10	593·7	0·84	17.	35 54	43 16	61 08	717·6	1·19
12.	19 47	332 45	10 13	591·1	0·86	19.	35 01	48 06	60 27	711·1	1·18
13.	21 08	333 04	12 12	594·7	0·85	21.	36 18	52 48	62 46	712·4	1·27
14.	23 06	334 39	15 12	603·0	0·84	22.	37 07	55 20	63 13	719·7	1·27
15.	25 02	336 50	17 45	611·9	0·83	23.	37 40	59 15	63 54	721·8	1·29
16.	26 12	339 12	19 50	608·2	0·85	24.	37 46	63 38	63 51	723·0	1·28
17.	25 56	340 34	22 01	623·0	0·82	25.	37 50	66 04	64 10	719·2	1·31
18.	27 02	339 15	23 46	629·0	0·81	26.	38 15	69 31	65 04	713·1	1·38
19.	27 51	337 51	24 57	628·5	0·82	27.	39 31	74 00	65 02	713·8	1·37
20.	29 06	337 17	26 20	619·8	0·86	29.	39 27	81 00	66 07	Not obs.	
21.	29 30	336 40	26 02	619·1	0·86	Oct. 2.	40 12	91 14	67 26	703·6	1·55
22.	30 18	337 05	27 11	621·0	0·86	4.	41 02	99 29	68 02	699·6	1·61
23.	30 47	337 26	28 05	621·4	0·87	5.	41 18	103 53	68 15	698·3	1·63
24.	32 01	338 08	30 13	623·0	0·88	10.	42 32	122 36	70 19	711·4	1·73
25.	33 39	341 00	32 46	628·2	0·89						

C.

The observations of Commander Edward Barnett, R.N., were made with a dipping instrument, the construction of which was favourably spoken of by S. H. Christie, Esq., in a paper read before the Royal Society in April 1833, as noticed in Commander Barnett's report. The needle having been used for the intensity as well as for the dip, the dip results are derived from observations in four positions only of the instrument, the poles of the needle not being changed in the course of each observation.

D.

The observations of Captain Edward Belcher, R.N., are part of a very extensive series, principally on the coast and islands of the Pacific Ocean, which is still in progress, and of which the details will be more conveniently given on a future occasion, when the series is completed.

E.

Observations on the dip made by Commander John Wickham, R.N., with a circle by Robinson of six inches in diameter furnished with two needles.

Date.	Needle.	Poles. α direct. β reversed.	Dip.	Station.
1837.		° ′	° ′	
June 26.	1	α 69 18	69 14	Plymouth, Athenæum.
		β 69 10		
	2	α 69 04	69 09	
		β 69 15		
July 20.	1	α 57 45	57 46	Santa Cruz, Teneriffe.
		β 57 47		
	2	α 57 55	57 45	
		β 57 35		
July 22.	1	α 57 50	57 49	Santa Cruz, Teneriffe.
		β 57 48		
	2	α 57 39	57 49	
		β 57 58		
Aug. 21.	1	α 5 42	5 32	Bahia.
		β 5 22		
	2	α 6 06	5 37	
		β 5 09		
Sept. 26.	1	α −53 03	−52 56	Cape of Good Hope, Dock Yard.
		β −52 48		
	2	α −53 11	−52 51	
		β −52 31		
Oct. 7.	1	α −52 50	−52 57	Cape of Good Hope, The Admiral's Garden.
		β −53 04		
	2	α −52 35	−52 52	
		β −53 09		
Nov. 22.	1	α −62 27	−62 23	Swan River, Pier.
		β −62 19		
	2	α −62 34	−62 24	
		β −62 14		
1838.				
Feb. 5.	1	α −42 58	−43 07	Point Swan.
		β −43 15		
	2	α −42 47	−43 07	
		β −43 27		
Mar. 24.	1	α −43 29	−43 26	Port Usborne.
		β −43 23		
	2	α −43 48	−43 26	
		β −43 04		
1838.				
April 15.	1	α −41 28	−41 30	Port George IV.
		β −41 31		
	2	α −41 08	−41 29	
		β −41 50		
June 15.	1	α −62 28	−62 24	Swan River Pier.
		β −62 21		
	2	α −62 38	−62 26	
		β −62 14		
July 12.	1*	α −69 57	−70 29	Hobart Town.
		β −71 01		
	2*	α −70 58	−70 19	
		β −69 41		
13.	1	α −70 20	−70 24	Government House.
		β −70 27		
	2	α −70 19	−70 26	
		β −70 32		
Oct. 25.	1	α −62 56	−62 53	Sydney. Fort Macquarrie.
		β −62 49		
	2	α −63 04	−62 52	
		β −62 40		
Dec. 11.	1	α −69 07	−69 09	Three Hummock Island. Bass's Strait.
		β −69 10		
	2	α −69 00	−69 08	
		β −69 16		
1839.				
Mar. 21.	1	α −62 48	−62 45	Dunheved. Captain King's Observatory.
		β −62 41		
	2	α −62 57	−62 45	
		β −62 34		
April 16.	1	α −62 41	−62 48	Sydney. Fort Macquarrie.
		β −62 56		
	2	α −62 42	−62 52	
		β −63 02		

* With a circle and needles made by Dollond.

F.

Captain Frederick Beechey's observations of the dip were made with a six-inch circle and needle by Robinson.

Date.	Poles. α direct. β reversed.	Positions of the needle. a.	a'.	a''.	a'''.	Dip.	Station.
1836.							
Feb. 20.	α	−13° 28′·2	−13° 00′·8	−13° 27′·5	−13° 15′·8	−12° 54′	Rio de Janeiro.
	β	−10 33·3	−13 34·1	−13 18·6	−12 37·1		
Mar. 18.	α	−23 42·7	−21 09·9	−22 54·7	−22 06·2	−21 40	St. Catherine.
	β	−19 34·3	−20 25·3	−22 17·8	−21 06·5		
	α	−23 38·5	−20 57·5	−22 55·5	−22 06·2	−21 39	
	β	−19 51·6	−21 11·2	−22 11·1	−20 25·3		
July 2.	α	−38 27·7	−37 55·6	−38 47·9	−37 19·4	−37 03	Valparaiso.
	β	−36 06·2	−36 06·8	−35 56·1	−35 49		
	α	−38 45·4	−37 16·9	−38 20·9	−38 02·2	−37 07	
	β	−36 10·1	−35 54·8	−36 17·4	−36 06·9		

G.

The observations of Captain Charles Drinkwater Bethune, R.N., were made with an instrument by Robinson of six inches diameter, furnished with two needles. The usual series of eight readings in different positions of the circle and needle were made, and the results are a mean of those given by the two needles. The readings in the different positions accorded well, as is usually the case in Robinson's instruments. Captain Bethune's report contains the details of the eight readings in each observation, but without specifying the positions to which they respectively belong. The results at the three stations employed in this paper are a part of an extensive series, chiefly in India and the Pacific Ocean, which will come under notice at a subsequent period, when observations now making in those quarters of the globe shall be completed; the details of Captain Bethune's observations at the three stations now employed will be more conveniently given at that time.

H.

The observations of Commander William Allen, R.N., were made with a circle of Jones with two needles, in both of which the readings, when the poles were changed, differed considerably. The results have been deduced by the known formula

$$\tan I = \frac{\tan M + \tan M'}{2},$$

wherein M is a mean of the readings in the usual four positions with the poles direct, and M′ the same with the poles reversed.

The present amount of the secular change of the dip in the Gulf of Guinea and its vicinity is so considerable, as to render it necessary to apply a correction to Captain Allen's results obtained in the year 1833, to make them correspond to the mean epoch of the observations which the map represents, or to the beginning of 1837. From the best inference I am able to draw, in the absence of precise data, the north dip is diminishing, and the south increasing, in the vicinity of the islands of Fernando Po and St. Thomas, at an annual rate certainly exceeding half a degree. At Captain Allen's stations in the Quorra the rate of decrease (of north dip) is probably less, but is still very great. The correction applied has been, for three years, at the annual rate of half a degree for the islands in the Gulf of Guinea, and a quarter of a degree for those in the Quorra: the results so corrected are inserted in the map. At Ascension the secular change probably does not exceed 8′ or 10′*: also Captain Allen's observations at that island were made a year later than at the other stations; they have not therefore been corrected for epoch. The discrepancy in the results of different observers at Ascension and of the same observer in different situations in the island are well known to be considerable, and are naturally accounted for by its volcanic character. The mean of Captain Allen's results agrees well with Captain FitzRoy's in 1836.

Date.	Needle.	Poles. α direct, β reversed.	Positions of the needle.				Dip.	Reduction to Epoch.	Dip in 1837.	Station.
			α.	α′.	α″.	α‴.				
1833.			° ′	° ′	° ′	° ′	° ′	° ′	° ′	
Aug. 21.	2	α	7 22	8 29	6 59	8 33	11 53	−0 45	10 58	Stirling.
		β	14 25	16 27	17 20	14 59				
Mar. 22.	1	α	9 43	10 58	10 25	10 14	11 34			
		β	12 29	13 52	12 50	12 00				
Oct. 10.	2	α	10 05	10 17	9 53	10 55	13 51	−0 45	13 06	Egga.
		β	16 55	18 28	17 27	16 22				
Nov. .	1	α	− 0 45	+ 0 20	− 0 39	− 0 10	0 48	−1 30	− 0 42	Fernando Po.
		β	+ 1 55	+ 2 26	+ 1 18	+ 1 57				
Nov. .	2	α	− 0 55	+ 0 28	+ 0 31	− 0 10	0 48			
		β	+ 0 57	+ 2 33	+ 1 51	+ 0 52				
Dec. 27.	1	α	− 8 26	− 8 30	− 7 13	− 8 52	− 7 44	−1 30	− 9 14	Isla de Rolhas.
		β	− 7 15	− 7 04	− 6 46	− 7 52				
1834.										
Jan. 17.†	1	α	1 03	0 30	1 59	1 48	2 31	0 00	1 57	Ascension.
		β	3 06	4 37	3 25	3 41				
Jan. 21.‡	1	α	2 15	2 19	1 12	0 40	2 24			
		β	3 35	3 28	3 57	2 46				
Jan. 24.§	1	α	0 37	0 18	1 1	1 15	0 57			
		β	2 05	2 30	3 10	3 03				

I.

Captain Wickham's intensity observations were made with Hansteen's apparatus, and two needles Y and Z. The following Table contains their times of horizontal vibration at the stations, dates, and temperatures specified, taken, with Captain Beau-

* Voyage of H.M. ships Adventure and Beagle, vol. i. p. 527.

† On the sea-beach. ‡ At the spot where Captain Foster observed. § On the summit of the Green Mountain.

FORT's permission, from Captain WICKHAM's Report in the Hydrographic Office. The experiments to determine the effect of differences of temperature on the times of vibration do not appear to have been yet made, and are probably deferred until Captain WICKHAM's return to England at the conclusion of the service on which he is still employed. The provisional results which are here computed are therefore necessarily uncorrected for temperature.

Station.	Date.	Needle Y.		Needle Z.		Dip.
		Time.	Therm.	Time.	Therm.	
		s	°	s	°	° ′
Plymouth	June 26, 1837.	806·5	71	883·3	72	69 12
Santa Cruz	July 21, 1837.	691·7	87	750·6	88	57 48
Bahia	Aug. 31, 1837.	610·9	86	660·5	88	5 35
Cape of Good Hope	Sept. 27, 1837.	725·3	65	781·9	73	−52 53
Swan River	Nov. 22, 1837.	655·1	79	702·4	79	−62 23
Point Swan	Feb. 5, 1838.	578·9	93	632·9	93	−43 07
Port Usborne	March 24, 1838.	584·1	99	627·2	99	−43 26
Port George IV.	April 15, 1838.	577·6	94	619·6	93	−41 29
Swan River	June 15, 1838.	662·7	64	710·8	63	−62 25
Hobart Town	July 16, 1838.	742·2	54	795·7	54	−70 25
Sydney	Oct. 25, 1838.	661·2	71	709·1	71	−62 53
Three Hummock Island	Dec. 11, 1838.	724·7	70	778·3	69	−69 08
Dunheved	March 22, 1839.	664·1	78	712·4	80	−62 45
Paramatta	April 4, 1839.	660·9	67	708·4	67	−62 50
Sydney	April 16, 1839.	661·8	70	709·9	69	−62 50

The observations of this series furnish tests of the steadiness of the magnetic condition of the needles, or of the changes undergone by them in this respect, during two intervals, viz. from the 22nd of November 1837 to the 15th of June 1838, at both which dates they were vibrated at Swan River; and from the 25th of October 1838 to the 16th of April 1839, at both which dates they were vibrated at Sydney. In the second interval neither needle appears to have sustained any change deserving of notice; the times of vibration in October 1838 and in April 1839 differing only within the limits of the known fluctuations of the horizontal force at the same station on different days, and even on the same day. Between the observations at Swan River in November 1837 and June 1838, both needles appear to have lost force, equivalent in the case of Y to $7^{s}{\cdot}6$ in $662^{s}{\cdot}7$, and in that of Z to $8^{s}{\cdot}4$ in $710^{s}{\cdot}8$. During the remaining portions of the series, viz. from June 26, 1837, to November 22, 1837, and from June 15, 1838, to October 25, 1838, the observations furnish no direct or independent means of testing the needles; but an attentive consideration of their times of vibration leads to the inference, that Y must have lost magnetism considerably between June and November 1837, and that the loss took place in all probability in the early period of the voyage, or between Plymouth and Santa Cruz; whilst in the case of Z, there is no indication of any other loss than that shown to have taken place by the observations at Swan River. Admitting and allowing for that loss, and supposing no other to have taken place, the values of the intensity given by this needle,

at stations where the values are approximately known by the concurrence of other observers of recent date, are as follows, proceeding from Plymouth as a base station:

Cape of Good Hope . . .	1·030; and by other observers . .	1·016*
Hobart Town	1·830; and by other observers . .	1·814†
Sydney	1·691; and by other observers . .	1·675‡.

Captain WICKHAM's values, it may be seen, are *in excess*, which is a difference of a contrary nature to that which would have been occasioned by a *loss* of magnetism; we may therefore conclude, with much probability, that needle Z sustained no notable loss of magnetism during the whole of its employment, except that which the observations at Swan River show to have taken place between November 1837 and June 1838: the results with this needle, contained in the next Table, have been computed accordingly.

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In the case of needle Y, which appears to have lost magnetism between Plymouth and Santa Cruz, but to what amount the observations supply no certain means of deciding, we may take as base-stations, from the other extremity of the series, Sydney and Hobart Town, where we have already seen that the values of the intensity are approximately known, viz. 1·675 and 1·814. Allowing the loss of 7°·6, evidenced by the observations at Swan River, the results computed with this needle at Santa Cruz, and all the subsequent stations, accord well with those of needle Z, and with those of other observers; whence we may infer with much probability, that between July 1837 at Santa Cruz, and April 1839 at Sydney, needle Y sustained no other loss of magnetism than that deducible from the observations themselves. The results with this needle contained in the next Table have been computed accordingly.

The loss of magnetism between the observations at Swan River in November 1837 and June 1838, has been regarded as a gradual and uniform loss during the whole interval, in the absence of any satisfactory evidence to the contrary.

The results thus computed and inserted in the following Table must be considered as strictly provisional; the *absolute* measurements of the intensity about to be executed at the Cape of Good Hope by Lieut. EARDLEY WILMOT, R.A., and at Sydney and Van Diemen's Land by Captain JAMES ROSS, R.N., will supply important stations of reference in the ultimate revision of this series; based on exact measures at these stations, and corrected for the variations of temperature, Captain WICKHAM's observations may be expected to give the relative values of the intensity at the intermediate stations, with, what would now at least be considered, a high degree of precision.

* Cape of Good Hope	FITZROY	1836.	1·014	1·011.
	FRANKLIN	1836.	1·008	
† Hobart Town	FITZROY	1836.	1·817	1·814.
	FRANKLIN	1837.	1·810	
		1838.		
‡ Sydney.	DUNLOP	1831.	1·665	1·675.
	FITZROY	1836.	1·685	

Santa Cruz	Y = 1·284;	Z = 1·270;	Mean = 1·277
Bahia	= 0·879;	= 0·877;	= 0·878
Cape of Good Hope	= 1·032;	= 1·030;	= 1·031
Swan River	= 1·647; 1·643;	= 1·665; 1·661;	= 1·654
Point Swan	= 1·341;	= 1·306;	= 1·324
Port Usborne	= 1·337;	= 1·343;	= 1·340
Port George IV.	= 1·329;	= 1·339;	= 1·334
Hobart Town		= 1·830;	= 1·830
Sydney		= 1·694; 1·693;	= 1·693
Three Hummock Island	= 1·788;	= 1·799;	= 1·793
Dunheved	= 1·657;	= 1·671;	= 1·664
Paramatta	= 1·681;	= 1·697;	= 1·689

K.

These observations were made with a needle belonging to myself, which Sir John Franklin took out with him to Van Diemen's Land, on his proceeding to assume the government of that island. It was one of those which Mr. David Douglas had employed in magnetic observations in North America and the Sandwich Islands, and had preserved its magnetism unchanged for several years antecedent to Sir John Franklin's departure. The following account of its times of vibration at Hobart Town shows that its magnetism was equally steady after its arrival there, and affords presumptive evidence that it had been so in the interval. This presumption is corroborated by the remarkable agreement of the intensities obtained with it at the Cape of Good Hope and Hobart Town, with those of Captain FitzRoy in 1836 at the same places. The account is copied from a memorandum in my possession received from Sir John Franklin. A second needle which accompanied it proved too unsteady in its magnetism to yield any useful results.

"Rates of Major Sabine's intensity needle six inches long, suspended by a fine thread of unspun silk.

"Dublin, July 20th, 1836: Provost's Garden, Trinity College, dip 70° 59′·5: therm. 59°·3: 10 vibrations in 57·88 seconds. Observer, Major Sabine.

"Cape of Good Hope, November 1836: observatory, dip 52° 38′: 10 vibrations in 50·182 seconds: therm. 74°. Observer, Lieut. Thomas Burnett, R.N.

"Hobart Town, January 24th, 1837: in the Garden at Government House; 10 vibrations in 49·88 seconds: therm 64°. Observer, Lieut. Thomas Burnett, R.N.

"Hobart Town, May 4th, 1838: in cottage-garden, ten feet from west window: 10 vibrations in 50·970 seconds: therm. 53°. Observer, Captain Charles Drinkwater Bethune, R.N.

"Hobart Town, May 21st, 1838: at the Stone in the Government Garden: 10 vibrations in 50·033 seconds: therm. 63°·5. Observer, Captain Charles Drinkwater Bethune, R.N.

"Hobart Town, July 18th, 1838: Government House: 10 vibrations in 50·28 seconds: therm. 57°·25: dip, observed at the same spot with two of Mr. Fox's instruments, 70° 31'·5. Observer, Commander John Wickham, R.N."

The correction for differences of temperature at the times of vibration of this needle, experimentally determined, is found by the formula

$$T - T' = \cdot 0003\, T\, (t - t');$$

in which T' is the observed time of vibration corresponding to the actual temperature t', and T the corrected time corresponding to the standard temperature t. The corrected times of vibration at 60° Fahr. are as follows:

Dublin	57·89 seconds.
Cape of Good Hope	49·97
Hobart Town, January 1837 . .	49·82
Hobart Town, May 4, 1838 . . .	51·07
Hobart Town, May 21, 1838 . .	49·98
Hobart Town, July 18, 1838 . .	50·32

Whence the values of the intensity are as follows, Dublin being 1·399*:

Cape of Good Hope . . .	1·008
Hobart Town	1·810

The values determined by Captain FitzRoy in 1836† were,

Cape of Good Hope . . .	1·014
Hobart Town	1·817

L.

Mr. Fox's series of intensity results in France, Switzerland, and Germany, were obtained in the summer of 1838, with a four-inch needle deflected by the weights specified in the subjoined Table. The angles are half the sum of the deflections to the right and to the left, and are corrected for temperature at the rate of 2' for one centesimal degree, determined experimentally for the actual angles. In reference to these observations, (and to those of the dip in the Table in page 137.) Mr. Fox remarks, that he always waited till the needle was at rest, and then repeatedly moved it, by turning a little the concentric disk and bracket from behind, reading off again and again when the needle had settled after the application of gentle friction. This he generally did at least three times before he noted the results, so that in fact the

* Magnetic Survey of the British Islands, British Association Report, 1838, p. 191.

† Variations of the Magnetic Intensity, British Association Report, 1837.

observation at each station might be derived, with few exceptions, from the result of thirty or forty readings, or upwards.

Station.	Weights.	Angles of deflection.	Intensity. London = 1·000.		= 1·372.	Date.	Locality.
	grs.	° ′				1838.	
London	2·0	48 34	1·000	1·000	1·372	May 22.	Mean result at three stations.
	2·1	51 55	1·000			June 4.	
	2·2	55 33	1·000			June 8.	
Rotterdam	2·0	49 16	·9894	·9891	1·357	May 18.	Field west of the city.
	2·1	52 45	·9889				
Arnheim	2·0	49 33	·9852	·9841	1·350	May 18.	Open space outside the gate.
	2·1	53 08	·9839				
	2·2	57 00	·9832				
Cologne	2·0	50 08	·9768	·9780	1·342	May 17.	Open space S. of Cathedral.
	2·1	53 30	·9792				
Manheim	2·0	50 16	·9749	·9754	1·338	May 15.	Garden of Palace.
	2·1	53 50	·9750				
	2·2	57 38	·9763				
Basle	2·0	50 43	·9686	·9685	1·329	May 11.	Place of St. Pierre.
	2·1	54 23	·9683				
	2·2	58 21	·9687				
Berne	2·0	51 28	·9584	·9602	1·318	May 9.	Platform near the Cathedral.
	2·1	55 00	·9608				
	2·2	59 03	·9615				
Geneva	2·0	51 33	·9573	·9594	1·316	May 1.	Outside the city wall at the back of the Hôtel des Bergues.
	2·1	55 04	·9601				
	2·2	59 07	·9608				
Annecy	2·0	51 49	·9538	·9545	1·310	April 30.	Near the lake.
	2·1	55 35	·9542				
	2·2	59 39	·9556				
Nismes	2·0	52 48	·9474	·9467	1·299	April 23.	A garden S.E. of the city.
	2·1	56 52	·9460				
Puy de Dôme	2·0	52 22	·9526	·9517	1·306	April 11.	East side; on lava.
	2·1	56 23	·9512				
	2·2	60 43	·9514				
Nevers	2·0	51 16	·9673	·9672	1·327	April 9.	Open space in front of the barracks.
	2·1	55 03	·9663				
	2·2	59 00	·9680				
Fontainbleau	2·1	54 20	·9749	·9761	1·339	April 6.	English garden at the Palace.
	2·2	58 06	·9773				
Paris	2·0	50 28	·9781	·9794	1·343	March 29.	Garden of the Ecole des Mines.
	2·1	54 01	·9787				
	2·2	57 43	·9813				

Note supplied by Mr. Fox.

"At the first nine stations in this Table, a few fibres only of unspun silk were employed for suspending the weights; but at the last five, a very fine silk thread was used for this purpose, which caused greater leverage on the grooved wheel, and consequently greater deflection of the needle, on which account it was found, after a series of comparative experiments, that ·0060 should be added to the decimals expressing the intensity, which has accordingly been done in the results entered in the Table."

POSTSCRIPT.—*April* 8, 1840.

Since the above communication was read to the Society, I have received letters from Captain JAMES CLARK ROSS, commanding the Antarctic Expedition, dated from St. Helena, February 9, 1840, stating that he finds the observations of the magnetic dip and intensity made at sea with Mr. Fox's instrument succeed far beyond his most sanguine expectation. I venture to lay before the Society the following extract from one of his letters, because it is likely to convey, more strongly than any comments of my own could do, a conviction of the value of a class of observations, which it was one of the principal objects of this paper to recommend to more general practice.

"Our Fox's needles are doing their work admirably in both ships: you would be somewhat surprised to see the interchange of signals between the two ships when the dip comes to the *same minute*, and it is seldom more than a few minutes apart when corrected for the deviation due to each vessel; and even this small difference partly occurs from our only at present knowing but imperfectly the values of the corrections. We have however ample means for their determination, and we shall eventually be able to make all the necessary corrections. I feel satisfied that on board ship much of the disturbing influence can be done away with, and the exact value of the remainder can be determined; can we say the same of observations on shore, even under the most favourable circumstances?"

V. *A Supplement to a Paper entitled "Remarks on the Theory of the Dispersion of Light, as connected with Polarization." By the Rev.* BADEN POWELL, *M.A. F.R.S. F.G.S. F.R.Ast.S., Savilian Professor of Geometry in the University of Oxford.*

Received February 6,—Read March 5, 1840.

IN a paper inserted in the Philosophical Transactions* I endeavoured to elucidate what appeared to me an important point in the wave-theory, viz. the connexion (first pointed out by Mr. TOVEY) between certain conditions with respect to the *arrangement* of the ætherial molecules, and the *rectilinear* or *elliptic* form of the *vibrations*; thus affording a *criterion* of their nature, in the respective cases, by the evanescence, or finite value, of certain terms in the equations.

Since the publication of that paper I have been led to review the subject in connexion with the valuable illustrations given by Mr. LUBBOCK of the views of FRESNEL†, according to which it appears that the criterion just mentioned requires a material modification. I think it necessary therefore to submit to the Royal Society this short Supplement, in order to point out in what way the conclusions in my paper will be affected by these considerations.

In the investigation last referred to it is established,

1. That every system of molecules, constituted as here supposed, has at every point three axes of elasticity, at right angles to each other.

2. That if these axes be taken as the axes of coordinates, then, in the fundamental equations of motion, deduced as in my paper, we shall have

$$\Sigma \{\psi r \Delta y \Delta z\} \Delta \eta = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

and all terms of the same form in like manner $= 0$; so that the equations (13.) of my paper are reduced to

$$\left.\begin{aligned} \frac{d^2\xi}{dt^2} &= \Sigma \{(\varphi r + \psi r \Delta x^2) \Delta \xi\} \\ \frac{d^2\eta}{dt^2} &= \Sigma \{(\varphi r + \psi r \Delta y^2) \Delta \eta\} \\ \frac{d^2\zeta}{dt^2} &= \Sigma \{(\varphi r + \psi r \Delta z^2) \Delta \zeta\} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

* 1838, Part II.

† London and Edinburgh Journal of Science, November, 1839.

With the equations in this form we easily pursue the investigation of the dispersion formula; and this condition is, in fact, assumed by M. CAUCHY, by Mr. KELLAND and others, though without explicit reference to the axes of elasticity.

In the case of elliptically polarized light we are obliged to adopt a peculiar method. We have here to consider not, as in the common case, a *rectilinear* displacement ϱ, and its resolved parts ξ, η, ζ, but a *curvilinear* displacement, which is the result of two virtual rectilinear displacements acting at right angles to each other, and in a plane to which the ray is perpendicular; and one of which is always retarded behind the other by an interval b, which we consider as an arc less than π. In this case, therefore, we must proceed (as in my paper) by making one of the coordinate axes, as x, coincide with the ray, (or, more correctly and generally, the normal to the wave surface,) whence $\xi = 0$, $\Delta\xi = 0$, &c. The first of the three equations (2.) disappears, and η, ζ coincide with the components which give the elliptic motion and are of the forms

$$\left.\begin{aligned} \eta &= \Sigma\, \alpha \sin (n\,t - k\,x) \\ \zeta &= \Sigma\, \beta \sin (n\,t - k\,x + b) \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (3.)$$

In pursuing the investigation on the principles now referred to, Mr. LUBBOCK shows that in this case we have always

and

$$\left.\begin{aligned} &\Sigma\, \{\alpha \psi r\, \Delta z\, \Delta y \sin (k\, \Delta x)\} = 0 \\ &\Sigma \left\{\alpha \psi r\, \Delta z\, \Delta y \sin^2 \left(\frac{k\, \Delta x}{2}\right)\right\} = 0 \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (4.)$$

Thus upon the whole we have (in my notation)

$$\left.\begin{aligned} \Sigma\, \{\alpha\, q \sin 2\theta\} &= 0 \\ \Sigma\, \{\alpha\, q\, 2 \sin^2 \theta\} &= 0 \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (5.)$$

$$\left.\begin{aligned} \Sigma\, \{q\, \Delta\zeta\} \quad &= 0 \\ \Sigma\, \{q\, \Delta\eta\} \quad &= 0 \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (6.)$$

$$\left.\begin{aligned} \frac{d^2\eta}{dt^2} &= \Sigma\, \{p\, \Delta\eta\} \\ \frac{d^2\zeta}{dt^2} &= \Sigma\, \{p'\, \Delta\zeta\} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (7.)$$

From the equation to the ellipse obtained from equations (3.), and since, in elliptic polarization, all the ellipses included in the summation must be equal and similar with their axes parallel, the semiaxes being respectively $\alpha \sin b$, and $\beta \sin b$, where $\alpha = A \sin i$, and $\beta = B \sin i$, it follows that we have α, β, and b, constant for all the ellipses. Hence, the factors involving those terms become common multipliers to the several sums.

Hence, following the same investigation as in my former paper*, we arrive at formulæ corresponding to (25.) (26.) (27.) (28.), whence there results,

$$\left.\begin{aligned} 0 &= -\Sigma\{\alpha p \sin 2\theta\} \\ 0 &= \Sigma\{p' \sin 2\theta\} \\ -n^2 &= \Sigma\{p' 2\sin^2\theta\} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (8.)$$

and the formula corresponding to (41.) gives

$$\sin b = \frac{0}{0} \quad \ldots\ldots\ldots\ldots \quad (9.)$$

Hence, it follows that in the case where the ray coincides with an axis of elasticity, the criterion of elliptic polarization ceases to be applicable.

* In the formula (17.) of my former paper, an oversight was discovered, too late to be corrected, in the *sign* of the last term, which should be + instead of −. This error runs through all the succeeding forms, and causes the introduction of the factor (cos 2 b) in (29.) (30.) and (31.); this factor should be unity. This error, however, does not in the least affect the ulterior results.

Also in p. 255, line 7 from bottom, *for* $\beta = 0$, *read* $b = 0$.

VI. *Additional Note to the Eleventh Series of Researches on the Tides.* By the Rev. W. Whewell, *B.D. F.R.S., Fellow of Trinity College, Cambridge.*

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THE tide observations which I recorded and discussed in my eleventh memoir on that subject, were laid before the Royal Society, because, though the different series of observations were both brief and imperfect, the features of the tide phenomena as there exhibited were novel; and it appeared desirable to put them on record with a view to future comparison with other places. I have now to notice other observations which I have received from another region, and which display similar features in a still more remarkable manner. These, with the results of a few other sets of observations, which may, I trust, hereafter be of use, I beg to lay before the Society, as an Appendix to my eleventh memoir on the subject of the Tides.

The principal tide observations which I now bring forward are those for which I am indebted to the Russian Admiral Lütke. These observations were made in 1827 and 1828 by the officers and men of the *Seniavine* corvette, commanded by the (then) Captain Lütke. From the account given me of the mode of observing, it appears that they were made with proper apparatus and with great care and perseverance, as is indeed sufficiently shown by the observations themselves. At one place (Petropaulofsk in Kamtchatka) the height of the surface was carefully observed every ten minutes day and night; and when near its maximum, every two minutes. And it is proper to remark, that this great care and labour, which would have been superfluous at most places, was necessary in this instance. If the observations had not been thus continued, they would not have enabled us to detect the very curious laws of the phenomena which I have now to describe.

I shall first state the peculiar features of the tide phenomena shown by these observations; and I shall afterwards add such portions of the registers and calculations as seem requisite to confirm and illustrate what I shall state.

1. *Petropaulofsk*, in the Bay of Avatcha, peninsula of Kamtchatka, latitude 53°·1 N., longitude 158° 44′ E. Observations made in October 1827, June 1838, October and November 1828.

On laying down these observations, in our usual manner, in curve lines, it appeared (as at Bassadore, already noticed in this memoir,) that the *high* water is affected in its *time* by a very large diurnal inequality, while the *height* is only slightly affected by an inequality of that kind. This diurnal inequality of the time reaches the enormous amount of above *four hours*; thus the intervals (between moon's transit and

high water) on October 11 and 12, 1827, were as follows: $5^h\ 38^m$, $1^h\ 39^m$, $5^h\ 20^m$, $0^h\ 56^m$; on June 23 and 24, 1828, they were $7^h\ 9^m$, $3^h\ 9^m$, $7^h\ 21^m$, $2^h\ 51^m$; on October 15 and 16, 1828, they were $5^h\ 13^m$, $2^h\ 0^m$, $6^h\ 7^m$, $2^h\ 46^m$; showing an alternate increase and diminution to the extent I have mentioned. The greatest alternate inequalities of the heights of high water during the series of observations which I have mentioned, were something more than a foot.

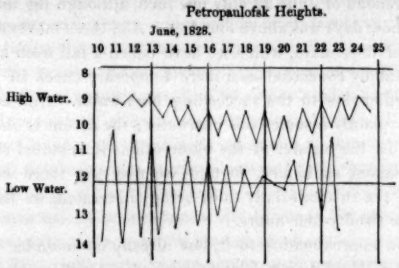
But when we examine the observations of *low* water, we find the case marked by additional features, which render it still more curious. For, in the low waters, the diurnal inequality of the times appears indeed, but is neither so large nor so regular as the inequality of the times of high water; it amounts at some periods to *one hour*, but not often to more. The diurnal inequality of the *height* of low water, on the other hand, is much larger than that for high water, reaching the amount of three, or even four feet; and this, in a tide of which the whole rise, from the lowest to the highest, rarely exceeds five feet.

The diurnal inequality depends, as is well known, upon the moon's declination; and its maximum and its disappearance have been found, at most places hitherto examined, to follow at a short interval (one or two days) the maximum and the vanishing of the moon's declination. If we examine the Petropaulofsk observations with regard to this point, we find that the greatest and most regular of the diurnal inequalities which I have noticed, the inequality of the time of high water and of the height of low water, correspond with the moon's declination; they have their maximum when the declination is greatest, whether north or south, and pass from positive to negative on the very day on which the moon crosses the equator. Using the phrases which have previously been employed on this subject, the *epoch* of the diurnal inequality is zero, and the effect of the moon's declination reaches Petropaulofsk without any delay or retardation.

But this view of the laws of the tides at this place, which might otherwise be accepted without difficulty, is extremely perplexed and interfered with by the other parts of the diurnal inequality,—the inequality of heights at high water and of times of low water. For though these inequalities are not so large or so regular as the others, still they are sufficiently marked and steady to allow their laws to be seen beyond doubt. And it appears, not only that the epoch of these parts of the inequality does not agree with that of the others, but that these two inequalities *alternate* with the other two, vanishing when the other reach their maxima, and showing their maxima when the others vanish.

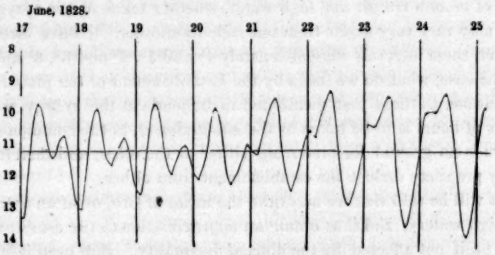
This is a very perplexing circumstance; for we cannot doubt that the diurnal inequality depends upon the moon's not moving in the equator; and therefore, how this inequality should affect the height of high water and the time of low water most, at that period at which the moon is in the equator, it is difficult to conceive. As the series of observations upon which my inferences are founded are but short (three series of a few weeks each), it might be doubted perhaps whether they are sufficient

to establish so curious a rule as this. I think no one looking at the observations thrown into the form of curves, will doubt that in them a rule of this kind really prevails; but I quite agree with the opinion, that in order to fix the rule with exactness a larger series of careful observations is requisite. In order that the reader may judge for himself, I annex the figures given by the observations for June 1828.



I shall proceed to point out a few additional inferences which offer themselves as resulting from the Petropaulofsk tidal phenomena, before I proceed to the observations made at other places.

Single-Day Tides.—The tides of Petropaulofsk show more clearly than any that have yet been examined, the manner in which the diurnal inequality may be so large as to lead to the appearance of only one tide in the twenty-four (lunar) hours. This will be very readily seen, if we follow the motion of the surface of the water for a few days, which is done in the accompanying figure.



The vertical lines in this figure represent the heights of the surface, and the horizontal space measures the time, each division being a day. It will be seen that there are on each day two high waters, excepting the 21st, when the second high water passes over to the 22nd, in consequence of the lunar being larger than the solar day. The heights of the high waters are alternately greater and less; as are also, in a still

greater degree, the heights of the low waters. And thus some of the high waters are depressed, and some of the low waters elevated, till there is little vertical distance between the two. On the 17th of June the rise from low to high water in the afternoon was only eight inches, although the rise in the forenoon had been four feet. In the same manner, the fall from high to low water in the forenoon of June 22 was only two inches, and in the forenoon of June 23 only one inch, although the intermediate fall in the afternoon of those days was above four feet. And these movements of the surface in the forenoons of those days, which we have called a fall from high to low water, are in fact more simply conceived as a mere temporary check in the rise of the water from the preceding low to the succeeding high water. And accordingly on June 24 the movement plainly assumes this character; the ascent is checked, but no descent takes place. In the register of the observations it is stated that "from 6^h to 8^h the surface remained stationary, so that we may say there was no low water." And thus one of the two half-daily tides being obliterated, we have at this period only one tide in the twenty-four hours.

This phenomenon, or an approximation to it, has already come under our notice in the tides of Sincapore, of the Keeling Islands, and of Bassadore; and has been noticed by others in other places. It is to be recollected, however, that this reduction of the two half-day tides to a single-day tide takes place only at a particular period of each lunation, depending upon the declination of the moon. When therefore a traveller has such a phenomenon brought under his notice, he should recollect that by pursuing his tide observations for a few days with assiduity, he will find the single-day tide resolve itself into the usual case of two daily tides.

"*The establishment*" *ceases to be applicable.*—The diurnal inequality affects the time of high water at Petropaulofsk to a very large amount, as we have seen; and hence the interval of moon's transit and high water, whether taken at the syzygy or at any other time, may vary very much from one tide to another. I have already noticed cases in which these intervals were alternately 1^h and 5^h, 2^h and 6^h, 3^h and 7^h. Now this being the case, what do we mean by the Establishment of the place? Supposing such observations as those just mentioned to be made at the syzygy, which of the two numbers of hours is to be taken as the *establishment*, in the common sense of the term? There is no ground for preferring either to the other, and therefore we cannot with any propriety deduce the establishment from either.

Perhaps it will be said that we may take the mean of two, or of an even number of successive high waters; and thus obtain an approximation to the establishment, such as it would be if not affected by the diurnal inequality. But even if a mean establishment were so obtained, it could not be applied. Thus if it appeared that the lunitidal interval for the time of syzygy, cleared of diurnal inequality, were five hours, still, as the cycle of the diurnal inequality is entirely different from the time between syzygy and syzygy, the interval at syzygy may not be affected at all by the inequality, or it may be affected by the greatest positive, or by the greatest negative value. It

may thus be five hours, or seven hours, or three hours. And this uncertainty deprives the establishment of all utility in such cases as the one before us, except we also take into account the diurnal inequality.

If, after laying down the lunitidal intervals as ordinates, we draw a line cutting off the diurnal inequality, according to the method which we have previously employed, we find that the semimenstrual line gives intervals oscillating between the (mean) values of $3^h\ 39^m$ and $5^h\ 25^m$. Hence the mean lunitidal interval or *corrected* establishment (using terms explained in former memoirs) is $4^h\ 32^m$: and the maximum amount of the semimenstrual inequality (positive and negative) is 53^m, and its total variation during a semilunation is 106^m; which is larger than the variation at most places. Hence we see of what very wide changes the lunitidal interval at Petropaulofsk is susceptible. If we take the maximum of the diurnal inequality (positive or negative) at two hours, the lunitidal interval may vary from $1^h\ 39^m$ to $7^h\ 25^m$; and thus the constancy of the establishment is quite obliterated. And even in this statement, we have not taken account of the effects of the parallax of the moon, and the effect of her declination upon the diurnal mean.

Diurnal Tide Wave.—As I have shown in former memoirs, we may represent the usual diurnal inequality at any place as the effect of a tide wave arriving at the shore once a day and superimposed upon the semidiurnal tide wave. We are naturally led to ask whether such a mode of representation is applicable to the tides now under consideration. The features which the diurnal inequality exhibits at Petropaulofsk are not, for the most part, inconsistent with such a representation. Thus, that the inequality should affect high water and low water very differently, is easily explained. Nor is there any difficulty in accommodating the hypothesis of the diurnal wave to one of the most curious of the laws which we have discovered; namely to this, that the inequality affects in the largest degree the *time* of high water and the *height* of low water. It is perhaps worth while to show this. In order to simplify the case as much as possible, I shall state and prove the following proposition.

If the maximum of the diurnal wave coincides with the minimum of the semidiurnal wave, there will be a diurnal inequality of the *time* of high water, and of the *height* of low water, depending upon the proportion of the maxima of the two waves; but there will be no diurnal inequality in the *height* of high water or in the *time* of low water.

Let t be any time measured in (lunar) half-days from a given high water; then the height (above mean water) of a semidiurnal tide may be represented (omitting inequalities) by $a \cos 2\pi t$. This gives high water when t is 0, 1, 2, or any whole number, and low water when t is $\frac{1}{2}, \frac{3}{2}$, or the half of any odd number. On the same supposition a diurnal tide might be represented by $b \cos \pi t$, which is a maximum when t is 0, 2, 4, and a minimum when t is 1, 3, 5, &c. But since the maximum of the semidiurnal and of the diurnal tide do not necessarily coincide, the latter may be represented

more generally by $b \cos(\pi t - \beta)$, which, when β is a quadrant, becomes $b \sin \pi t$. In this case the maximum of the diurnal wave coincides with the minimum of the semidiurnal: and the compound wave is $a \cos 2\pi t + b \sin \pi t$.

In this case it is proposed to find the diurnal inequality. For high and low water we have, by the rule for maxima,

$$-2a \sin 2\pi t + b \cos \pi t = 0, \text{ or } b \cos \pi t = 4a \sin \pi t \,.\, \cos \pi t.$$

Hence

$$\cos \pi t = 0, \text{ or } \sin \pi t = \frac{b}{4a}.$$

The former value gives for t the values $\frac{1}{2}, \frac{3}{2}, \frac{5}{2}$, &c., which belong to the time of low water, and show that the time of low water is not altered by the diurnal wave.

The other value gives for t a certain fraction x, and also the numbers $1 - x$, $2 + x$, $3 - x$, &c.; these belong to the time of high water, and show that the time of high water is affected by a diurnal inequality, of which the maximum, positive and negative, is x.

The height of low water is obtained by substituting, for t, the values $\frac{1}{2}, \frac{3}{2}, \frac{5}{2}$, &c. which give the *depressions*, $a - b$, $a + b$, $a - b$, &c., showing that the height of low water is affected by a semidiurnal inequality, of which the maximum is b positive and negative. The height of high water is obtained by substituting, for t, x, $1 - x$, $2 + x$, $3 - x$, all which give the values $a \cos 2\pi x + b \sin \pi x$, or $a + \frac{b^2}{8a}$; showing that there is no diurnal inequality of height in the high water.

If the maximum of the diurnal wave do not coincide either with the maximum or the minimum of the semidiurnal, the compound wave will be $a \cos 2\pi t + b \cos(\pi t - \beta)$; and there will be inequalities both of time and of height, both for high and low water. And the inequalities at high and low water will bear certain proportions depending upon β and $\frac{b}{a}$; and conversely, if the proportions of the high-water and low-water diurnal inequalities of time and height be known, the quantities β and $\frac{b}{a}$ may be found.

But all this is applicable so long only as the inequalities of high and of low water coincide in their cycles, reaching their maxima and vanishing together; and as we have seen that, instead of doing this, they reach their maxima and vanish alternately, we are compelled to give up (in this instance) this mode of representing the diurnal inequality by means of a diurnal wave.

Under the present aspect of the subject, I do not conceive that we can pursue this examination further with advantage, till we possess a longer series of careful observations of the Petropaulofsk tides. The singular laws which the tides there follow makes it a matter of interest to science to have these laws still further established

and analysed; the knowledge that the case does thus deserve further inquiry, we owe to the very great exactness and perseverance with which the observations were made by Admiral Lütke in 1827 and 1828.

2. Port of *Novo-Arkhangelsk*, Isle of *Sitkhæ*, Norfolk Sound, on the north-west coast of America, latitude 57° 2′ N., longitude 135° 18′ west of Greenwich. Observations made in July 1827*.

The Isle of Sitkhœ is nearly in the same latitude as Kamtchatka, on the opposite side of the Northern Pacific, the distance of the two coasts being in that part about 66 degrees of longitude. The differences which have been found to prevail in the phenomena of the tides even in places almost adjacent, leave us without any good ground of expectation that the tides of these two coasts will be found to resemble each other, even in their general features. It is found, however, that some of the most curious of the circumstances which mark the Petropaulofsk tides, appear also in those of Novo-Arkhangelsk.

The Sitkhœ tides exhibit a very great diurnal inequality both in the heights and in the times. The amount of this inequality at high water reaches an hour (half an hour positive and negative); at low water it is somewhat less. The diurnal inequality of the height of high water is two feet and a half; at low water its maximum effect amounted to five feet, the greatest rise from low water to high water being about fifteen feet.

Thus we see that at Sitkhœ, as in Kamtchatka, there is a very large diurnal inequality which affects the time of high water more, and the height less, than those of low water. The motion of the surface will be in its general features the same in this as in the former case, although it does not appear, so far as the observations before us can show, that one of the two semidiurnal tides is ever obliterated.

I may add, that it appears also, so far as we can judge from a short series of observations, (a month, with interruptions,) that the maximum and zero of the high-water diurnal inequalities alternate with those of low water, which we have already noticed as so perplexing a circumstance at Petropaulofsk.

The diurnal inequality being allowed for, we obtain a semimenstrual inequality of the mean tide at Sitkhœ, which is tolerably regular. According to our discussion, the greatest and least lunitidal intervals are $12^h\ 10^m$, and $13^h\ 25^m$ from the lunar transit of the preceding half-day; the mean on the corrected establishment being $12^h\ 49^m$, and the total amount of the semimenstrual inequality 75^m.

Under these circumstances we cannot doubt that the tides of Kamtchatka and Sitkhœ are to be explained in the same way, whatever the explanation may be; and probably the same features extend in some measure over the whole of the North Pacific.

* Some additional observations were made in 1832 and 1833, but too interruptedly to be of much service in these discussions.

3. *Port de la Coquille*, Isle of Ualan, Caroline Archipelago, latitude 5° 21′ N., longitude 163° 5′ east of Greenwich. Observations made in December 1827.

These observations were made for about nineteen days, with interruptions. They exhibit some of the same features as the two preceding places; a very large diurnal inequality, both of the heights and times, and both at high and low water. In this case the inequality of the heights appears to be greatest for high water. Also its maximum appears to alternate with the maximum of the inequality for low water; but the series of observations is too short and incomplete to determine this point with certainty.

Having allowed for the diurnal inequality, we obtain a very large semimenstrual inequality. The lunitidal intervals appear to oscillate from $3^h\ 30^m$ to $6^h\ 16^m$. These very wide changes may throw suspicion upon the exactness of the observations; yet the changes are so regular and continuous, as greatly to remove such suspicions. According to these results the mean lunitidal interval or corrected establishment is $4^h\ 53^m$.

4. *Port Lloyd*, Isle *Bonine-Sima*, latitude 27° 5′ N., longitude 142° 5′ west of Greenwich. Observations made May 1828.

Port Lloyd has a very large diurnal inequality, affecting both the times and the heights; it affects most the high-water times and the low-water heights. In the inequality of the heights, the maxima for high water and low water appear to alternate; in the times, this is more obscure; but we cannot expect clear evidence on this head, the observations including only nine days.

For the same reason it is impossible to state with any precision the establishment of this place. The maximum lunitidal interval (cleared of diurnal inequality) is $8^h\ 42^m$; and from the course of the curve which represents the observations, it appears certain that the semimenstrual inequality of times is large. If this inequality be two hours, the mean lunitidal interval will be $7^h\ 42^m$.

The observations sent by Admiral Lütke contained an account of the Establishment of several other places in the North Pacific; but, as I have already stated, the term *establishment* becomes in the highest degree vague, and almost unmeaning, when applied to seas in which the phenomena of the tides are such as are described in the present paper.

P.S. From the account of Admiral Freycinet's voyage, it appears that at several places in the North Pacific, the tides, as observed in that expedition, exhibit features similar to those here noticed.

Petropaulofsk.

	Moon's Transit.	High Water.		Interval.	Low Water.		Interval.
		Times.	Heights.		Times.	Heights.	
1827.	h m	h m	ft. in.	h m	h m	ft. in.	h m
Oct. 1 A.M.	7 57	1 50	16 10	5 53	6 43	18 1	10 46
P.M.	8 24	52	16 11	4 28	7 20	19 7	10 56
2 A.M.	8 50	2 8	16 8	5 18	8 13	18 9	11 23
P.M.	9 16	2 24	16 8	5 8	8 21	19 0	11 5
3 A.M.	9 42	2 48	16 3	5 6	9 5	19 0	11 23
P.M.	10 7	3 24	16 1	5 17	9 42	18 6	11 35
4 A.M.	10 31	3 32	16 1	5 1	10 15	19 9	11 44
P.M.	10 56	4 44	16 10	5 48	10 8	18 3	11 12
5 A.M.	11 20	3 50	16 0	4 30	10 42	20 1	11 22
P.M.	11 44	5 20	16 2	5 36	10 44	18 4	11 0
6 A.M.	8	4 32	16 3	4 24	11 1	20 5	10 53
P.M.	32	6 3	16 0	5 31			
7 A.M.	56	5 10	16 0	4 14	3	17 11	11 31
P.M.	1 20	7 11	16 1	5 51	22	20 6	11 26
8 A.M.	1 45	5 26	16 1	3 41	11 40	17 8	10 20
P.M.	2 10	7 54	16 3	5 44	45	20 4	11 0
9 A.M.	2 34	5 51	16 2	3 17	17	17 4	10 7
P.M.	2 57	8 18	16 2	5 21	1 5	20 3	10 31
10 A.M.	3 21	6 19	16 3	2 58	1 30	17 2	10 33
P.M.	3 45	8 51	16 6	5 6			
11 A.M.	4 10	7 6	16 9	2 56	2 6	17 6	10 21
P.M.	4 34	10 12	17 0	5 38	2 30	20 2	10 20
12 A.M.	4 58	6 37	17 0	1 39	3 7	17 7	10 33
P.M.	5 22	10 42	16 3	5 20	3 7	19 9	10 9
13 A.M.	5 46	6 42	16 7	56	3 54	17 0	10 32
P.M.	.....	.....	.....	.....	4 59	19 6	11 13
14 A.M.	6 9	12	16 11	6 3	5 20	17 7	11 11
P.M.	6 31	10 42	17 3	4 11	4 35	18 11	10 4
15 A.M.	6 54	1 9	16 8	6 15	7 11	18 0	12 21
P.M.	7 17	11 59	17 4	4 42	6 49	19 3	11 32
16 A.M.	7 39	1 15	17 2	5 36	7 26	18 10	11 47
P.M.	8 2	2 7	17 7	6 5	7 46	19 0	11 44
17 A.M.	8 25	1 38	17 0	5 15	7 52	18 11	11 27
P.M.	8 49	2 44	17 0	5 55	8 28	18 7	11 39
18 A.M.	9 12	2 25	16 9	5 13	8 59	19 9	11 47
P.M.	9 35	3 22	17 0	5 47	9 7	18 5	11 32
19 A.M.	9 59	2 27	16 9	4 28	9 50	20 3	11 51
P.M.	10 23	4 39	16 6	6 16	10 2	17 11	11 39
20 A.M.	10 48	2 56	16 4	4 8	10 17	20 7	11 29
P.M.	11 14	5 38	16 0	6 24	10 39	17 5	11 25
21 A.M.	11 40	3 27	16 1	3 47	10 43	20 9	11 3
P.M.	6	6 28	15 9	6 22	11 15	17 0	11 9
22 A.M.	34	3 58	15 8	3 24	11 23	20 10	10 49
P.M.	1 2	6 53	15 1	5 51			
23 A.M.	1 31	4 41	15 10	3 10	6	17 2	11 4
P.M.	2 1	7 44	16 3	5 43	44	21 2	11 13
24 A.M.	2 30	4 59	16 1	2 29	26	17 2	10 25
P.M.	2 59	8 36	16 5	5 37	51	21 3	10 21
25 A.M.	3 28	5 45	16 2	2 17	1 30	17 2	10 31
P.M.	3 58	9 38	16 2	5 40	2 7	20 10	10 39
26 A.M.	.....	.....	.....	.....	2 53	17 0	10 55
1828.							
June 10 A.M.							
P.M.	10 2	.....	.....	.....	9 21	14 1	11 19
11 A.M.	10 26	4 46	9 1	6 20	9 48	10 1	11 22
P.M.	10 51	2 34	9 3	3 43	10 15	14 1	11 24

Petropaulofsk. (Continued.)

	Moon's Transit.	High Water.		Interval.	Low Water.		Interval.
		Times.	Heights.		Times.	Heights.	
1828.	h m	h m	ft. in.	h m	h m	ft. in.	h m
June 12 A.M.	11 16	5 30	8 11	6 14	10 58	10 1	11 42
P.M.	11 41	2 37	9 4	2 56	10 48	14 2	11 7
13 A.M.	6	5 56	9 0	5 50	11 55	10 4	11 49
P.M.	30	3 11	9 8	2 41	11 18	14 2	10 48
14 A.M.	55	6 41	9 2	5 46	36	10 7	11 41
P.M.	1 19	3 38	9 10	2 19	11 53	14 2	10 34
15 A.M.	1 42	7 18	9 4	5 36			
P.M.	2 5	5 2	10 1	2 57	54	10 9	11 12
16 A.M.	2 28	7 45	9 6	5 17	29	13 9	10 24
P.M.	2 51	5 29	10 3	2 38	1 28	11 1	11 0
17 A.M.	3 13	8 6	9 5	4 53	45	13 5	9 54
P.M.	3 35	6 19	10 6	2 44	2 13	11 2	11 0
18 A.M.	3 57	8 30	9 6	4 33	1 11	13 0	9 36
P.M.	4 19	7 59	10 7	3 40	2 54	11 5	10 57
19 A.M.	4 41	9 10	9 5	4 29	1 57	12 5	9 38
P.M.	5 2	9 6	10 10	4 4	3 28	11 9	10 47
20 A.M.	5 23	9 59	9 6	4 36	2 40	11 11	9 38
P.M.	5 45	10 50	10 8	5 5	4 11	12 1	10 48
21 A.M.	6 7	10 4	9 2	3 57	2 45	11 1	9 0
P.M.	.....	.....	.....	.....	5 27	12 5	11 20
22 A.M.	6 29	1 22	10 4	6 53	3 28	10 6	8 59
P.M.	6 53	10 33	8 11	3 40	6 29	12 11	11 36
23 A.M.	7 17	2 26	10 1	7 9	4 32	10 2	9 15
P.M.	7 41	10 50	9 1	3 9	7 8	13 7	11 27
24 A.M.	8 6	3 27	9 8	7 21			
P.M.	8 32	11 23	9 1	2 51	8 1	14 2	
25 A.M.	8 58	4 17	9 6	7 19			
Oct. 6 A.M.							
P.M.	9 37	.....	.....	.....	9 12	14 3	11 35
7 A.M.	9 59	3 12	12 1	5 13	9 44	14 8	11 45
P.M.	10 21	4 24	11 10	6 3	10 1	13 9	11 40
8 A.M.	10 43	3 39	11 10	4 56	10 9	14 10	11 26
P.M.	11 5	5 11	11 7	6 6	10 31	13 5	11 26
9 A.M.	11 27	3 58	11 7	4 31	10 38	15 3	11 11
P.M.	11 49	5 31	11 5	5 42	11 2	13 2	11 13
10 A.M.	13	4 39	11 6	4 26	11 3	15 3	10 50
P.M.	37	6 21	10 11	5 44	11 47	12 4	11 10
11 A.M.	1 1	4 50	11 1	3 49	11 59	15 6	10 58
P.M.	1 26	7 0	11 2	5 34			
12 A.M.	1 51	5 4	11 2	3 13	4	12 5	10 38
P.M.	2 17	8 21	11 5	6 4	1 0	15 7	11 9
13 A.M.	2 43	5 19	11 2	2 36	12 0	12 3	9 43
P.M.	3 10	8 56	11 6	5 46	1 14	15 7	10 31
14 A.M.	3 38	5 59	11 5	2 21	1 53	12 5	10 43
P.M.	4 6	9 19	12 0	5 13	1 42	16 0	10 4
15 A.M.	4 34	6 34	11 10	2 0	2 50	12 7	10 44
P.M.	5 2	11 9	12 1	6 7	2 37	15 10	10 3
16 A.M.	5 30	8 16	12 2	2 46	3 25	12 10	10 23
P.M.	5 59	11 32	11 9	5 33	3 47	15 4	10 17
17 A.M	6 27	9 9	12 6	2 42	6 0	12 9	12 1
P.M.	.....	.....	.....	.....	4 55	14 11	10 28
18 A.M.	6 54	28	12 1	5 34	6 58	13 5	12 4
P.M.	7 22	11 38	12 7	4 16	6 9	14 8	10 47
19 A.M.	7 49	54	12 0	5 5	8 0	14 2	12 11
P.M.	8 16	1 38	12 4	5 22	7 23	14 3	11 7
20 A.M.	8 43	1 22	11 9	4 39	8 6	14 6	11 23
P.M.	9 10	2 37	11 11	5 27	8 2	13 9	10 52

Petropaulofsk. (Continued.)

	Moon's Transit.	High Water.		Interval.	Low Water.		Interval.
		Times.	Heights.		Times.	Heights.	
1828.	h m	h m	ft. in.	h m	h m	ft. in.	h m
Oct. 21 A.M.	9 36	2 7	11 7	4 31	8 59	15 1	11 23
P.M.	10 2	4 12	11 1	6 10	9 13	12 5	11 11
22 A.M.	10 28	2 16	10 4	3 48	10 0	14 6	11 32
P.M.	10 54	4 36	10 5	5 42	10 13	12 4	11 19
23 A.M.	11 21	3 25	10 3	4 4	10 25	14 11	11 4
P.M.	11 47	5 33	10 1	5 46	11 4	11 9	11 17
24 A.M.	13	4 3	10 3	3 50	11 25	15 5	11 12
P.M.	39	6 35	10 2	5 56	11 35	11 10	10 56
25 A.M.	1 6	4 31	10 3	3 25	11 48	15 3	10 42
P.M.	1 32	7 9	10 2	5 37			
26 A.M.	1 59	5 0	10 8	3 1	55	11 9	11 32
P.M.	2 26	7 48	10 6	5 22	34	15 5	10 35
27 A.M.	2 52	5 17	10 10	2 25	1 42	11 9	11 16
P.M.	3 17	8 50	10 10	5 33	1 19	15 3	10 27
28 A.M.	3 42	6 49	10 11	3 7	2 30	11 11	11 13
P.M.	4 7	9 9	10 4	5 2	1 55	14 1	10 13
29 A.M.	4 31	6 37	11 7	3 6	3 3	12 1	10 56
P.M.	4 55	10 11	10 11	5 16	2 31	14 5	10 0
30 A.M.	5 18	8 6	11 7	2 48	3 44	12 2	10 49
P.M.	5 41	10 42	11 0	5 1	3 39	13 10	10 21
31 A.M.	6 4	9 25	12 0	3 21	5 49	12 5	12 8
P.M.	6 26	11 45	11 3	5 19	4 27	13 8	10 23
Nov. 1 A.M.	6 48	10 34	12 7	3 46	7 41	13 5	13 15
P.M.	.....	.....	.....	.....	5 44	13 9	10 56
2 A.M.	7 10	28	11 9	5 18	7 26	13 11	12 16
P.M.	7 31	1 29	12 8	5 58	6 14	13 6	10 43
3 A.M.	7 53	39	11 6	4 46			
P.M.	8 15	2 4	11 2	5 49	7 52	12 0	11 37
4 A.M.	8 37	1 36	10 3	4 59	8 11	13 6	11 34
P.M.	8 59	3 43	10 11	6 44	8 0	12 2	11 1
5 A.M.	9 21	1 39	11 1	4 18	9 35	14 10	12 14
P.M.	9 43	3 53	11 6	6 10	8 55	12 8	11 12
6 A.M.	10 6	2 17	11 4	4 11	9 36	15 2	11 30
P.M.	10 29	.....	.....	.....	10 32	12 5	12 3
7 A.M.	.....	2 44	11 5				

Port De Lloyd.

Lat. 27° 5′ N. Long. 142° 54′ E.

	Moon's Transit.	High Water.		Interval.	Low Water.		Interval.
		Times.	Heights.		Times.	Heights.	
1828.	h m	h m	ft. in.	h m	h m	ft. in.	h m
May 4 A.M.	3 29	11 23	7 2	7 54	4 43	4 5	1 14
P.M.	3 59	10 34	6 8	6 34	5 49	5 11	1 50
5 A.M.	4 28	12 53	7 2	8 25	5 35	4 7	1 7
P.M.	4 56	11 46	6 8	6 50	7 14	5 10	2 18
6 A.M.	.....	.....	.....	.....	7 2	5 0	1 38
P.M.	5 24	2 9	7 1	8 45	8 14	5 7	2 23
7 A.M.	5 51	1 53	6 6	8 2	8 8	5 0	1 50
P.M.	6 18	3 20	7 2	9 2	9 35	5 2	2 51
8 A.M.	6 44	3 7	6 9	8 23	9 30	5 1	2 20
P.M.	7 10	3 53	7 1	8 43	10 22	4 8	2 46
9 A.M.	7 36	4 27	6 10	8 51	10 31	5 2	2 30
P.M.	8 1	4 24	7 1	8 23	10 55	4 6	2 29
10 A.M.	8 26	5 17	7 0	8 51	11 28	5 2	2 38
P.M.	8 50	4 58	7 2	8 8	11 37	4 3	2 22
11 A.M.	9 15	5 56	7 4	8 41	11 36	5 4	1 56
P.M.	9 40	5 19	7 5	7 39			
12 A.M.	10 5	6 48	7 5	8 43	15	4 1	2 10
P.M.	10 29	6 6	7 4	7 37	25	5 6	1 56
13 A.M.	10 54	7 38	7 4	8 44	49	4 0	1 55
P.M.	11 19	6 24	7 2	7 5	49	5 6	1 30

Bay of St. Croix.

1828.							
Sept. 9 A.M.		9 30	9 6		4 30	4 11	
P.M.		10 5	9 1		4 15	4 7	
10 A.M.		10 35	10 1		4 45	4 4	
P.M.		10 30	10 6		5 0	4 9	
11 A.M.		10 35	10 4		4 45	4 7	
P.M.		11 10	10 6		5 15	4 7	
12 A.M.		11 10	10 5		5 30	4 7	
P.M.		11 40	10 11		6 30	4 5	
13 A.M.		11 35	10 1		6 0	4 6	
P.M.		.....	.....		7 0	4 6	
14 A.M.		15	11 0		7 0	4 4	
P.M.		7	9 7		7 15	4 2	
15 A.M.		1 10	10 10		7 0	4 6	
P.M.		50	9 10		7 30	4 7	
16 A.M.		1 37	11 5				

Port De la Coquille.

Lat. 5° 21′ N. Long. 163° 5′ E.

	Moon's Transit.	High Water.		Interval.	Low Water.		Interval.
		Times.	Heights.		Times.	Heights.	
1827.	h m	h m	ft. in.	h m	h m	ft. in.	h m
Dec. 12 A.M.		11 30	7 3		5 29	6 2	
P.M.		.....	.....		6 25	5 6	
13 A.M.		20	8 2		7 0	5 9	
P.M.		38	7 6		6 26	5 3	
14 A.M.		9	8 6				
P.M.		1 27	7 7		7 18	4 8	
15 A.M.		1 38	9 4		8 29	4 7	
P.M.		1 49	9 0		7 55	4 9	
16 A.M.		2 40	9 9		9 16	4 4	
P.M.		2 27	8 4		8 29	4 9	
17 A.M.		2 48	10 2		9 35	4 3	
P.M.		3 36	8 3		9 16	4 7	
18 A.M.		3 33	9 3		9 57	4 1	
P.M.		3 35	8 4		10 14	4 5	
19 A.M.		4 11	10 1		11 7	3 7	
P.M.		.....	.....		10 32	4 5	
20 A.M.		4 57	9 3				
P.M.							
21 A.M.		5 26	9 10				
P.M.		6 9	8 6		18	5 5	
22 A.M.		6 23	9 4		16	6 0	
P.M.		8 14	8 2		1 1	5 1	
23 A.M.		7 3	8 9		31	6 8	
P.M.		9 23	8 2		1 38	6 1	
24 A.M.		8 30	8 3		3 30	6 11	
P.M.		10 35	8 3		4 13	6 7	
25 A.M.		10 39	7 11		3 43	7 1	
P.M.		.....	.....		4 11	6 5	
26 A.M.		12 0	8 8		7 35	6 6	
P.M.		47	7 11		6 22	6 7	
27 A.M.		57	8 10		8 32	6 5	
P.M.		1 28	7 10		6 43	6 4	
28 A.M.		1 25	8 11		7 37	6 4	
P.M.		1 32	7 10		7 35	6 3	
29 A.M.		2 33	9 3		7 57	5 11	
P.M.		2 46	8 1		8 12	5 10	
30 A.M.		2 37	9 6		9 3	5 5	
P.M.		3 16	8 1		8 48	5 10	
31 A.M.		3 7	9 9				
P.M.							

Sitkhœ, Norfolk Sound, N.W. coast of America.

Lat. 57° 2′ N. Long. 135° 18′ W.

	Moon's Transit.	High Water.		Interval.	Low Water.		Interval.
		Times.	Heights.		Times.	Heights.	
1827.	h m	h m	ft. in.	h m	h m	ft. in.	h m
July 1 A.M.							
P.M.	5 20	5 44	13 0	12 22	11 53	9 1	6 10
2 A.M.	5 43	6 22	15 4	12 39			
P.M.	6 6	7 11	12 11	13 5			
3 A.M.	6 32	7 25	15 10	12 53	58	8 10	6 26
P.M.	6 59	8 47	13 3	13 48			
4 A.M.	7 26	8 17	16 8	12 51	1 55	9 2	6 29
P.M.	7 53	9 57	13 8	14 4	3 17	6 3	7 24
5 A.M.	8 21	9 19	16 9	12 58	3 5	9 5	6 45
P.M.	8 51	10 50	14 4	13 59			
6 A.M.	9 21	.....	.....	.....	4 10	9 5	6 49
P.M.	9 51	11 29	15 2	13 38			
7 A.M.	10 22	11 34	18 3	13 12	5 10	9 2	6 48
P.M.	.....	.....	.....	.....	6 1	4 0	7 8
8 A.M.	10 52	42	16 8	13 49	5 52	8 7	6 27
P.M.	11 25	.....	.....	.....	6 46	3 3	6 49
9 A.M.	11 57	1 22	15 8	13 25	6 40	7 8	6 14
P.M.	28	42	17 6	12 14	7 40	3 2	6 42
10 A.M.	58	2 10	16 2	13 12	7 34	7 8	6 7
P.M.	1 27	1 42	17 4	12 12	8 14	3 8	6 19
11 A.M.	1 55	2 48	16 2	12 53	8 34	7 4	6 12
P.M.	2 22	2 28	16 5	12 6	8 53	4 7	6 5
12 A.M.	2 48	3 35	16 3	12 47	9 24	7 6	6 10
P.M.	3 14	3 13	15 6	11 59	9 39	5 7	6 0
13 A.M.	3 39	4 9	16 1	12 39	10 16	7 7	6 13
P.M.	4 3	4 10	14 6	12 7	10 22	6 9	5 55
14 A.M.	4 27	4 52	15 7	12 25	11 22	7 7	6 32
P.M.	4 50	5 20	13 4	12 30	11 12	7 8	5 58
15 A.M.	5 14	5 24	15 1	12 10	11 55	7 10	6 18
P.M.	5 37	6 14	12 8	12 37			
16 A.M.	6 0	6 37	15 2	12 37	1 3	8 11	7 3
P.M.	6 23	7 58	12 8	13 35	1 29	7 10	7 6
17 A.M.	6 43	7 44	15 4	13 1	1 27	9 11	6 44
P.M.	7 10	9 8	13 1	13 58	2 57	7 9	6 48
18 A.M.	7 33	8 19	15 6	12 46	2 2	10 7	6 29
P.M.	7 57	10 6	13 4	14 9	2 42	8 0	6 45
19 A.M.	8 20	9 6	15 6	12 46	3 34	10 6	7 14
P.M.	8 44	11 13	13 9	14 29	3 47	8 0	7 3
20 A.M.	9 7	10 17	15 10	13 10	4 22	10 4	7 15
P.M.	9 31	11 33	14 1	14 2	5 20	6 3	7 49
21 A.M.	9 55	11 31	16 2	13 36	5 27	9 10	7 32
P.M.	.....	.....	.....	.....	5 18	7 10	6 59
22 A.M.	10 19	22	14 9	14 3	5 39	10 2	6 56
P.M.	10 43	11 39	16 8	12 56	6 22	6 0	7 15
23 A.M.	11 7	40	15 0	13 33	6 12	9 7	6 41
P.M.	11 31	14	16 8	12 43	6 53	5 10	6 59
24 A.M.	11 54	57	15 2	13 3	7 19	9 2	7 2
P.M.	17	1 30	14 9	13 13			
25 A.M.	40	1 46	15 2	13 6	7 55	8 9	6 52
P.M.	1 3	.....	.....	.....	8 4	5 11	6 38
26 A.M.	1 26	2 33	15 11	13 7	8 9	8 11	6 20
P.M.	1 49	.....	.....	.....	8 38	5 10	6 27
27 A.M.	2 11	3 49	15 7	13 38			
P.M.	2 33	3 29	14 11	12 56	9 59	7 1	7 3
28 A.M.	2 56	3 30	15 7	12 34	9 31	7 11	6 12
P.M.	3 19	3 29	14 6	12 10			

VII. *An Account of the Shooting Stars of* 1095 *and* 1243. *By Sir* Francis Palgrave, *K.H. F.R.S. &c.*

Received February 19,—Read March 5, 1840.

THE Chronicles of the Middle Ages frequently contain important notices of natural phenomena worthy of more attention than they have hitherto received. The narratives of these signs and wonders are, of course, frequently given with the involuntary exaggerations of ignorance and of fear: but at the same time they are usually characterized by sincerity. And in the description of the very remarkable appearance of the shooting stars of the 4th of April 1095, we have the testimony of a great number of independent witnesses in France and England, whose attention was particularly drawn to the phenomenon, from the circumstance of its being concurrent with the Crusade.

I will begin with the account in the Saxon Chronicle, which, as far as we can collect, was compiled at Peterborough; and, from their style and narrative, we can scarcely doubt that these venerable annals were the result of notes taken down contemporaneously with the events which they describe.

"1095. In this year Easter fell on the eighth day before the kalends of April; and on the night of the feast of St. Ambrose (that is to say the 2nd before the nones of April) there was seen nearly throughout all the land, and during all the night, manifold stars falling from the heavens: not by one or by two at a time, but so thickly that no man could reckon them."

Florence of Worcester describes the same appearance more shortly, by stating that on the 2nd of the nones of April, in the night, "stars were seen, as if falling from heaven."

The Chronicle of the Abbey of Anjou states, that "on Wednesday, the 2nd of the nones of April, the moon being twenty-five days old, stars were seen falling like a shower of rain from heaven upon the earth;" an expression of some importance when compared with the remarkable fact which we shall find given by the next authority.

This is Sigebert of Gemblours, who states, that "on the 2nd of the nones of April, about daybreak, very many stars were seen to fall at once from the heavens to the ground." One very large star thus fell in France; and a bystander, having noted the spot, "cast water upon it, which was raised in steam, with a great noise of boiling:" a description leaving no doubt that in this case the star was an aerolite.

The Chronicle of Verdun, by Hugh de Flavigny, states that "on Wednesday, the day before the nones of April, from the middle of the night until dawning, stars were seen falling."

The CHRONICLE of RHEIMS, giving the same date, describes the appearance as if all the stars in heaven were driven like dust before the wind; and this continued from cock-crowing until dawn.

ORDERICUS VITALIS, who lived in the monastery of Ouches in Normandy, grounds his narrative upon his own observations and those of GILBERT, Bishop of Lisieux, who professed medicine and astrology, and, as such, was a diligent observer of the courses of the stars. ORDERICUS gives exactly the same date as the other authorities, Wednesday the day before the nones of April; and says that "so many shooting stars were seen, and so thick were they together, that, if they had not shone, they might have been taken for hail." When GILBERT saw the phenomenon, he called up his assistant, WALTHER of Cormeilles, and explained to him that it portended the changes and wanderings of nations from kingdom to kingdom; and this was the explanation which the meteors generally received.

With respect to the shooting stars of 1293, they have not the same advantage of a number of witnesses; but our single authority, MATTHEW PARIS, the monk of St. Albans, is clear and precise. "1243. In this year, on the 7th of the Calends of August (26th July), being the 8th day of the new moon," says he, "the air being most pure and calm, so that the milky way was seen as in a clear winter's night, the stars were seen to fall from the heavens, darting this way and that. In one minute thirty or forty were seen to fall, two or three following in one track; so that if they had been true stars, not one would have remained in the heavens."

VIII. *Further particulars of the Fall of the Cold Bokkeveld Meteorolite.* By Thomas Maclear, *Esq. F.R.S. &c., in a Letter to Sir* John F. W. Herschel, *Bart. K.H. V.P.R.S. &c. &c. Communicated by Sir* John F. W. Herschel.

Received February 5,—Read March 5, 1840.

Royal Observatory, Cape of Good Hope,
November 20, 1839.

My dear Sir John,

As I did not receive any further communication relating to the Bokkeveld Meteor after my last communication to you on the subject, I resolved to undertake the promised journey in conjunction with our friend Watermeyer, to whom we are so much indebted in the inquiry. Accordingly we made up a waggon party, consisting of himself and two of his sons, with Lieut. Jacob of the Indian Survey, Mr. Bailey, Hon. E.I.C.S., and myself, and set out on the 9th instant, taking Worcester and Tulbagh in our way, for the purpose of consulting with Dr. Treuter and the Rev. Mr. Fahn, who had been engaged in collecting information.

On the 14th we took horses at Tulbagh, and proceeded to the house of Pieter de Toit in the Bokkeveld, where we slept. The next morning we visited the other people who had witnessed the phenomenon.

I inclose two sketches of the Cold Bokkeveld. No. 1. was sent to me by Dr. Treuter*. No. 2. is a copy of it with some corrections made on inspecting the place. Therefore the original is of no other use than as a voucher. The principal alteration refers to the position of Van Heeren's house (Van Erde as written by Dr. Treuter), which is not a joint habitation, but distant some seven miles from Barand Joosteen's.

I inclose also two letters received from Dr. Treuter, wherein he alludes to two explosions, and to two spots only where meteorolites fell, about twenty-five miles from each other.

With regard to the first, *no single individual in the Bokkeveld heard two explosions*, but persons beyond fancied they did; the second most likely was the echo among the mountains.

With regard to two spots only where meteorolites fell, I may remark that there was no witness in the interval of the line of flight between Joosteen's and De Toit's habitations; consequently it is impossible to say what may have fallen. These re-

* Plate VII. is the sketch No. 2. here referred to. A plate of No. 1. is not given, but the original is deposited in the archives.

marks are made with the view of explaining that which otherwise would appear contradictory to the description I am about to give.

There is also inclosed an interesting affidavit of one of the witnesses.

The Cold Bokkeveld is an irregular valley or basin, bounded by high rugged mountains of sandstone, accessible only by passes of the character of the French Hoek and Sir Lowry's, but rougher, and in some parts very steep. The entrance from Tulbagh is magnificent compared with any other place I have seen at the Cape.

Within the basin the ground undulates; some of the hills are high, and covered with groups of sandstone. The farms are principally situated at the west side, or near the base of the Schurfde Berg, and are partially screened from each other. Van Heeren's is low, Barand Joosteen's is on the east side of a hill, about seven miles from Van Heeren's, and Pieter de Toit's is about fifteen miles from the latter, according to the map. These three habitations are completely hidden from each other. The climate is comparatively cool; European cherries grow in the orchards abundantly.

It is not necessary to give the separate conversations in detail held with the inhabitants, for they all agree in the description of the phenomenon. Their attention was first excited by a violent explosion, followed by a rumbling noise, like the rattle from heavy waggons passing over hard ground. On looking up they saw a blue stream of smoke, like that proceeding from ignited gunpowder, passing in a direction from south-west to north-east*, viz. from the summit of the Schurfde Berg opposite Van Heeren's towards Pieter de Toit's. The noise caused several of the inmates to rush from their houses, some of whom obtained a partial glimpse of the phenomenon.

1st. At the instant, the son of Van Heeren was standing midway between his father's house and the Schurfde Berg, where he saw something fall to the ground, which he picked up. His mother saw another plunge into the swampy ground north-east of the house. This is the specimen of four pounds, the second sent to you. These two positions may be about a mile from each other, and were pointed out to us by Mrs. Van Heeren.

2ndly. Barand Joosteen, with two servants†, were near the mountain, viz. the hill E on the map, at the instant of explosion. They saw something descend to the ground, and where it struck, the grass smoked. The direction of this spot was pointed out to us by Barand Joosteen himself, and the specimen is the one I sent to Capt. Beaufort by H.M.S. Scout, Capt. Craige.

3rdly. A servant of Pieter de Toit's was standing near his master's house; he saw a substance fall to the ground about a mile below the garden, which he ran

* Although in Mr. Maclear's account the direction is S.W. to N.E., yet the plan, Dr. Treuter's second letter, and Mr. Fahn's letter (Phil. Trans. 1839, p. 85.), all agree in giving the direction N.W. to S.E. It would therefore appear that S.W. to N.E. in the text is an error of transcription.—S. H. C.

† See the Affidavit.

towards and brought to his master. The position of the spot was shown to us by the servant. I forgot to inquire to whom this specimen was given.

Each of these persons declares that the meteorolites were so hot on approaching them that they could not lay hold of them.

I have thus enumerated all that were *seen falling*. The curiosity of the people being excited they made further search. A mass was discovered on the road to the N.E. of Van Heeren's house beyond the swampy ground, but fractured, apparently in striking against the hard ground.

Barand Joosteen found a lump which separated into fragments on taking it up, owing, he thinks, to exposure to moisture. He had parted with some of it; the remainder, weighing nearly four pounds avoirdupois, he kindly presented to us.

Pieter de Toit found a quantity in fragments on the road near his house. Also another at position D, which was given to Mr. Fahn and is the first I sent to you. His son found a portion at position A. He showed us the places in person.

I am unable to estimate the total weight of the pieces that were found at De Toit's, for he had given them away to applicants. I understand they were brought to Cape Town, and that Mr. G. Thompson sent a piece of one to England.

I consider I am justified in the opinion that but a small proportion of the whole mass has been found, because six persons only chanced to be sufficiently near the track to witness the fall; two of them within a mile of each other. These were close together, but six or seven miles from the first, while only one was in the open air at De Toit's, seventeen miles further on, according to the map. The intervening ground is covered with brushwood and heath, similar to that on the floats about Tiger Berg, excepting a few cultivated patches of small extent.

The people, surprised by the presence of an imposing spectacle, the nature of which they could not well comprehend, would naturally follow up alone and without further consideration whatever first caught the eye, and this is supported by the fact of several portions being discovered at comparatively short distances from the witnesses, but unseen at the time of explosion. I may also remark, that beyond De Toit's house for a considerable distance in the direction of the line of flight, there is nothing but rugged mountain, without, I believe, inhabitants.

It appears that the portion which sunk in the moist ground near Van Heeren's is the one which gave rise to the exaggerated statement made to me, "of a piece having made a hole in the ground large enough to admit a table."

Kieviet declares in his affidavit, that the piece he picked up was originally seven or eight pounds weight. The specimen was sent to me by Dr. Truter, and I forwarded it to Captain Beaufort as I received it. The weight was rather more than four pounds; and, so far as I can recollect the shape of the fractured surface, I do not think much had been broken off. However, I believe this to be the largest specimen yet discovered.

In my first communication to you, I stated that the line of flight was north-westerly,

which was my impression after the conversation with Mr. Menzies, who at the time of the explosion was resting at the Blood River, ninety miles N.E.* of the Bokkeveld, in company with Mr. G. Thompson. But the witnesses in the Bokkeveld prove the contrary, which is further confirmed by the following extract from a letter of Mr. G. Thompson's to me.

"The meteor appeared to the best of my judgment to approach from the west with great velocity, and precisely similar to a Congreve rocket of large dimensions. The phenomenon expanded nearly overhead, and apparently not more than 3 or 400 feet high, dispersing in large globes the size of forty-two pound shot, of quicksilvery appearance. These fell for a few seconds towards the earth and vanished."

The latter part of this paragraph exhibits an optical deception as to distance, if "the separation into globes" was the explosion over the Bokkeveld. For be it remembered Mr. Thompson did not hear any noise, and the stations are separated by at least ninety miles.

If the object he saw was a portion of the meteor moving on beyond the Bokkeveld, most likely the dispersion would have been followed by a report. The first supposition is probable, and tends to prove that the object was much elevated when it exploded.

Mr. Thompson also learned that the explosion was heard within a few miles of Clanwilliam; on the Zonder-End River, and the Cape Downs!

With reference to the accuracy of the map, I verified the azimuth direction of the line of flight, as it was pointed out to us, with a small azimuth compass. The distance between De Toit's and the Schurfde Berg did not appear to us twenty-five miles, but on the whole it is sufficiently correct to explain a matter of this kind.

I forgot to mention that the witnesses described the heavens as being perfectly free from clouds and calm at the time.

If you think this communication, with Dr. Treuter's letters and Kievit's affidavit, worthy of the attention of the Royal Society, be pleased to place them on the table.

Believe me, dear Sir John,

Yours, ever faithfully,

Thomas Maclear.

To Sir J. F. W. Herschel, *Bart.*,
&c. &c. &c.

* From the plan, this again would appear to be an error of transcription, N.E. for S.E.—S. H. C.

Dr. TREUTER'S *Letters to* THOMAS MACLEAR, *Esq.*

Worcester, 4th June, 1839.

MY DEAR SIR,

I have great pleasure in acknowledging your letter of the 1st of May last, and feel much obliged for the copy of Sir J. HERSCHEL's interesting letter. Although I have been prevented returning you an earlier answer, I have not forgotten the main object of our present correspondence, and have already secured about four pound in Dutch weight of the meteorolite, and given directions to obtain all the fragments, which may be in possession of some individuals of this division, which I will give myself the pleasure to forward to you by the first fit opportunity which may offer. I am also in possession of an affidavit of a person who happened to be quite close to the spot where the burst took place and the meteorolite fell, and will be able to accompany it with a pretty correct sketch of the direction the meteor took. From all information I have been able to collect, it appears there are only two spots, distant about twenty-five miles from each other, where an explosion of the meteor occurred with the fall of stones. I should advise you not to make any public announcement of it in the papers, for the people will fancy that it is of such very great value that they may weigh it with gold.

Believe me, my dear Sir,

Most truly yours,

T. MACLEAR, *Esq.* J. TREUTER.
&c. &c. &c.

Worcester, 10th July, 1839.

MY DEAR SIR,

I have much pleasure in availing myself of this opportunity to forward to you herewith the fragments I have been able to collect of the meteorolite. From all the information I obtained on the subject of the meteor of the 13th of October 1838, of which the sensation was felt simultaneously over an extent of upwards of 150 miles, it appears that the falling of meteorolites was only observed at two places, distant from each other, as the bird flies, about twenty-five miles; and that the fall of these meteorolites was accompanied with an explosion resembling the loudest peal of thunder. The atmosphere was in such an agitation, that the windows of the houses were shaken as from a violent wind, and the noise produced was that of a swift passing waggon heavily laden. The first conclusion the people drew from it was that of its being a slight shock of an earthquake. On the spots where the meteor was observed, it appeared to take its course from north-west to south-east, with a loud hissing noise, producing a train of evanescent light. At the time, the sky was quite clear; no wind; the atmosphere sultry.

The inclosed sketch of that part of this division where the falling of the meteorolites has been observed, will I trust give you an accurate idea of its course: it appeared to come from over the summits of the Schurfde Berg. The accidental circumstance of a waggon being near the foot of that mountain, where the meteor passed over the heads of the people employed in collecting a load of fire-wood, has enabled me to obtain a deposition from one of these persons, which I inclose herewith.

Any fragments which I may be able further to collect I shall forward to you by an early opportunity.

Believe me, my dear Sir,

Most truly yours,

J. Treuter.

P.S. The large piece is of the first, the small one of the second explosion.—Vale!

Affidavit.

I, the undersigned Kieviet, Bastaard Hottentot, maketh oath, that about the month of October last year I was in the service of Barend Jooste, of the Field Cornetcy of the First Cold Bokkeveld District of Worcester. On the morning of the 13th of that month, my master, myself, and another Bastaard Hottentot named Jacob Rooy, proceeded in a waggon to the mountain at the back of my master's house to fetch wood. It was a fine clear morning; there were no clouds in the sky, and there was no wind. At about nine o'clock A.M., whilst we were busy loading the waggon with the wood, close to the foot of the mountain, we heard a strange noise in the air resembling the loudest thunder we had ever heard, and on looking up we perceived a stream passing over our heads, issuing a noise which petrified us with terror; a burst took place close to the waggon, when something fell and a smoke arose from the grass. My master sent me to look what it was that had fallen, when I found a stone quite warm, so much so that I could not hold it in my hands: I brought it to my master, I do not know what he did with it. This stone now produced to me is part of the one I took up; it might have been then about the weight of seven or eight pounds.

Sworn before me, at Worcester,
this 30th day of April 1839,
J. Treuter,

Mark of
✕
Kieviet,

As Witnesses,
James Rattray.

IX. *An account of Experiments made with the view of ascertaining the possibility of obtaining a Spark* before *the Circuit of the Voltaic Battery is completed.* *By* John P. Gassiot, *Esq.* *Communicated by* P. M. Roget, *M.D. Sec. R.S. &c.*

Received October 11,—Read December 19, 1839.

1. In presenting an account of the following experiments to the notice of the Royal Society, I am fully aware that the results are somewhat at variance with those obtained by philosophers whose names are so eminent in science, that it may appear presumptuous in me even to offer an opinion as to the correctness of their statements. The discrepancy may very possibly arise from error on my part; but as the experiments relate to facts and not opinions, I can only state that I present a faithful record of those performed by myself with the assistance of two or three private friends, and that this record is extracted from my note-book, wherein the experiments were entered from day to day as they were performed.

2. Until some recent experiments of Dr. Jacobi* no one appears to have disputed the correctness of Dr. Faraday's original experiment of a spark appearing *before contact*, or before the circuit of a voltaic battery was completed, when consisting of a single pair of plates.

3. That a spark does appear when the contact is made *on completing* the circuit of the voltaic battery, is well known to all who have experimented with that apparatus. Dr. Faraday has shown that this spark can be produced even with a single pair of plates†; and although, in the preface affixed to the octavo edition of his Researches, this philosopher considers, from the results obtained by Dr. Jacobi, he was in error as to the spark taking place before contact, yet, with the precaution which he has described, the spark will invariably appear whenever the circuit is completed.

4. Sir Humphry Davy, in his Elements of Chemistry‡, says a bright spark was produced of $\frac{1}{30}$th or $\frac{1}{40}$th of an inch in the open air, and in vacuo nearly half an inch, by using 2000 of Dr. Wollaston's plates.

5. Mr. Children informed us, that with 1250 pairs the spark was capable of passing through a distance of $\frac{1}{50}$th of an inch§.

6. Dr. Henry, in his Elements of Chemistry, expressly states that the galvanic fluid passes through air and certain *non-conductors* in the form of sparks.

7. In the edition of Turner's Chemistry, now in the progress of publication, it is stated‖ that, on approximating the wires of an active circle, a brilliant spark passed between them just *before* contact, as well as in the act of breaking contact.

* Philosophical Magazine, December, 1838, p. 401. † Philosophical Transactions, 1834, § 957.
‡ Page 152. § Philosophical Transactions, 1809. ‖ Page 153.

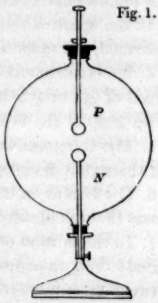
8. Professor Daniell, in describing some very curious and important experiments made with his constant battery when excited with strong acid solutions*, says, "That "a discharge may take place from the copper of one cell to the copper of the next, "when the regular circle is interrupted between the two, I had many opportunities of "observing with the powerful currents with which I had been experimenting; *for I "have frequently seen it pass in the form of a spark* when the cells had been too much "approximated in the air." And in his Introduction to the Study of Chemical Philosophy, § 725, this gentleman also informs us, that, when the energy of the battery has been elevated by the repeated impulses of a series, it will project through an interval of air in the form of the most dazzling fire.

9. With this array of authority before me, I certainly entered into the investigation with some diffidence: but the inquiry was important. Dr. Jacobi's experiments, to which I have already alluded, proved that with twelve platinum and zinc plates, excited with a solution of eight parts sulphuric acid to 100 parts of water, the spark could not pass through $\frac{1}{50,000}$th of an inch; and the question was now, whether using a more powerful apparatus this alleged spark was obtainable or not. Fortunately the researches of Professor Daniell have, in the constant battery†, placed an apparatus in the hands of the experimentalist which enables him to examine his results with that care and attention so indispensable in all philosophical pursuits, but which were quite incompatible with the acid batteries used in the time of Davy.

10. My first experiments were made with 160, and subsequently with 320 series of the constant battery, each series consisting of the usual elements of zinc and copper, each pair being placed in a half-pint porcelain jar, the exciting liquids, solutions of muriate of soda and sulphate of copper, separated from each other by brown paper. The effects from this battery were of the most brilliant description; but as they have been described elsewhere‡, it is only necessary for me to state that this arrangement was used in the following experiments.

11. Fig. 1. represents a glass globe which was attached to a good air-pump, the upper wire sliding through collars of leather air-tight, P and N representing the positive and negative electrodes of the battery (10.) The small balls attached to the two wires were then approximated, as near as the eye could determine, without being in actual contact; but no sparks could be observed even when the globe was exhausted by means of the air-pump.

Fig. 1.



12. In order to avoid as much as was in my power any error in the approximation of the electrodes, I had an instrument prepared which, in order to understand correctly the next series of experiments, it is necessary I should describe.

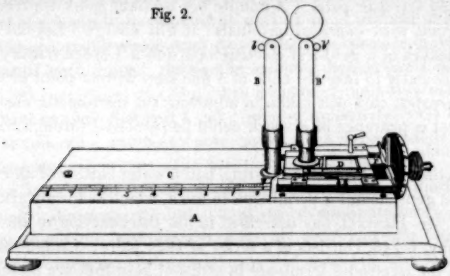
* Philosophical Transactions, 1837, p. 121.

† Ibid. 1836.

‡ Transactions of the Electrical Society.

Fig. 2. A represents the wooden base of the instrument: BB′ are two glass pillars about three inches high, on which are fixed two brass balls with binding screws attached, *b b′*, and by which discs or points of platinum, or small pieces of charcoal, may be attached: C is a wooden slide with an ivory scale divided into tenths of an inch, and by which the pillar B can be moved; D a metal slide (to which the pillar B′ is attached) with

Fig. 2.



an index and scale to tenths of an inch; and E a scale attached to the base of the instrument, graduated to $\frac{1}{50}$th of an inch: the metallic slide D is moved by a *micrometer screw*, F, to the circumference of which is attached a scale divided into 100 equal parts. One complete revolution of the screw F acts on the metallic slide D, and consequently on pillar B′, to the extent of $\frac{1}{50}$th of an inch; and as the circumference of the scale attached to the screw is divided into 100 equal parts, every division will indicate an action on the pillar B′ to the extent of $\frac{1}{5000}$th of an inch. For the purpose of reference, I will designate this instrument as the *Micrometer Electrometer*.

13. It will be seen by the preceding description of the instrument, that I was now enabled to measure a distance of $\frac{1}{5000}$th of an inch, with what correctness the following experiment will show. A magneto-electric machine (made to avoid the use of mercury, by having a metallic break attached to the armature, on which contact is broken, and where of course the usual spark appears as the armature is rotated,) was placed on the table; wires were then attached to the pillars B B′ by the binding screws *b b′* of the micrometer electrometer (12.), and these wires being also attached to the magneto-electric machine in such a manner as to form part of the circuit, on rotating the armature, a brilliant spark was invariably obtained on the *break* whenever the points or balls of the micrometer electrometer (12.) were in contact, but when separated to the $\frac{1}{5000}$th of an inch, as denoted by one division of the scale, the spark immediately disappeared; showing that when the circuit was divided by this minute space, a space not appreciable to the eye even when assisted with a powerful lens, the spark could not be obtained. By this experiment, I not only ascertained that the magnetic spark could not pass through the space of $\frac{1}{5000}$th of an inch, but, what was more important to me for the object I had in view, I proved the correct action of

my instrument; and I now purport describing the different arrangements of the voltaic battery with which I used it.

14. I have had in constant action for upwards of twelve months a voltaic battery consisting of the usual elements, zinc and copper, each pair being placed in a separate circular glass vessel containing about a quart of rain water. The entire battery is composed of 150 series or pairs. A minute but brilliant spark is perceivable on *completing* the circuit with charcoal terminals: it will charge a Leyden jar sufficiently to deflect the leaves of a gold-leaf electroscope, and a Leyden battery of twelve one-gallon jars sufficiently to fuse the ends of a fine steel wire or point of a penknife; but although the greatest care was taken in adjusting the micrometer electrometer (12.), not the slightest appearance of a spark could be perceived through the minute space of $\frac{1}{5000}$th of an inch.

15. Recollecting that Professor Daniell had a water battery of an extended series, I applied to that gentleman for permission to use it, which he kindly granted me. This battery, which Mr. Daniell has described in his Introduction to the Study of Chemical Philosophy, § 743, consists of a series of 1024 pairs; attractions as well as repulsions, when the electrodes terminate in strips of gold leaf, are plainly perceptible; a minute but distinct spark is seen on completing the circuit; but with the *battery alone the spark could not be obtained* through the space of $\frac{1}{5000}$th of an inch.

16. The property of the voltaic battery, particularly when extended to such a series as that of Mr. Daniell's (15.), to charge a Leyden battery is well known; and I accordingly found, when a battery of nine jars was introduced in the circuit, I obtained distinct sparks to a distance varying from $\frac{1}{5000}$th to $\frac{6}{5000}$ths of an inch. When the space was reduced to $\frac{1}{5000}$th or $\frac{9}{5000}$ths of an inch, the sparks appeared in quick succession, the intervals between the sparks varying from two seconds to twelve seconds; but when the space was increased to $\frac{4}{5000}$ths, it required at least two minutes before there was sufficient accumulation or power to force a passage for the spark through such a space.

17. Having been, by the invitation of Professor Daniell, witness on the 16th of February 1839 of the powerful effects obtained by a series of seventy of the large constant battery, I was induced to prepare 100 of precisely the same dimensions. On the first day I excited that battery (14th of July), and when I was favoured with the company of Dr. Faraday, I also excited 100 of the smaller cells already described (10.); *but neither with these two powerful batteries combined*, or separate, could any appearance of a spark be observed until contact was made and the circuit completed.

18. Dr. Faraday placed a silk handkerchief between the electrodes of the 100 large cells, and although the terminal balls or ends of the connecting wires were pressed together and then separated, and this repeated several times, no effect could be obtained through this minute space; or, in other words, one thickness of a silk handkerchief interposed between the electrodes of such a powerful battery was sufficient to prevent any perceptible action.

19. Those who had the pleasure of witnessing the experiments of Professor DANIELL, at King's College, when a series of only seventy pairs of his constant battery was used, will no doubt recollect the brilliant effects produced with this powerful apparatus, and may form some idea of those obtained by using nearly one third increase of power. Titanium, which had been previously given to me by Dr. FARADAY, was fused into a solid mass, and is now in that gentleman's possession; platinum was volatilized; and the flame from charcoal as well as from metallic electrodes was so intense as to render it indispensable that the eyes of those present should be protected by thick screens of black crape. On another evening sixteen feet four inches of platinum wire No. 20 iron gauge, was ignited to a red heat; and even this length might have apparently been extended had I had a greater quantity of that wire; but with such a powerful apparatus no effect could be obtained even through one thickness of a silk handkerchief.

20. It will be seen by the preceding experiments that I was unsuccessful in my attempts to obtain a direct spark, or what is usually denominated the striking distance, by the direct or *unassisted* action of any of the powerful batteries hitherto described in this paper; but that when a Leyden battery of nine jars was introduced in the circuit of the 1024 series (15. 16.), the spark passed through the space of $\frac{6}{1000}$ths of an inch.

It is necessary to explain, that, since making the experiments I am about to describe, my attention has been pointed to similar results obtained by Dr. FARADAY in his Ninth Series*; and had I been aware of them I should certainly have saved myself much useless time and expense. The results are however in every way confirmatory of the experiment of this philosopher†; but from the powerful apparatus I used the results obtained were proportionately more decided.

21. In the course of my previous experiments I had often introduced coils of copper ribbon and of copper wire in the circuit of the voltaic battery, without being enabled to obtain any indication of a spark before contact, although the brilliancy of the spark is thereby so much increased when the contact is broken. From the effects obtained by the water battery (15. 16.) of 1024 series, when the Leyden battery was introduced in the circuit, I was induced to try what effects could be obtained by what is generally called the secondary coil, or the action of an induced current. It is not, however, my intention to enter into the particulars of the numerous experiments made by myself, in conjunction with those private friends who kindly assisted me in this inquiry, but I will at once briefly explain the apparatus I ultimately used, and from which the most satisfactory results were obtained.

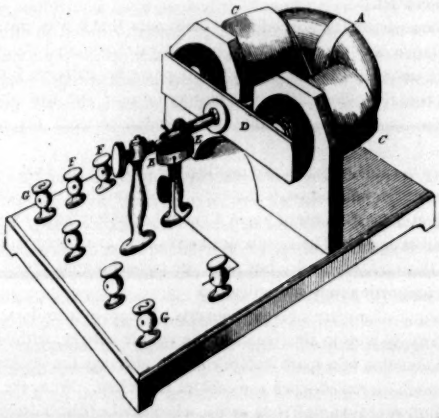
22. Twelve hundred and twenty-five iron wires, No. 20 gauge, each wire about twenty-one inches long, being previously well coated with resin, were bent into the usual horse-shoe form. 115 feet of No. 12 copper wire, well covered with cotton, and divided into three separate lengths, were then wound round the iron wires; bind-

* Philosophical Transactions, 1834. † Ninth Series. § 1090.

ing screws were attached for the purpose of making connexions with the voltaic battery, the whole forming an electro-magnet of considerable power. Over this thick wire 2268 feet of No. 30 copper wire, also well covered with cotton, were wound round, the ends being attached to separate binding screws, and forming a secondary coil. A bar of soft iron was placed so as to rotate within about $\frac{1}{50}$th of an inch from the ends of the iron wires, which ends were previously ground quite smooth, so as to expose an even surface; and connexions were made from the primary wire by means of two cups of mercury, so that the contact with the voltaic battery should be made and broken at every revolution of the iron bar.

The apparatus when in action is represented by fig. 3, and may be described as follows. A represents the iron wires forming the electro-magnet when the voltaic

Fig. 3.



current is passed through the primary coil; C, C the secondary coil; D the bar of soft iron; E, E cups of mercury connected with the primary coil; F, F binding screws to connect the primary coil with the voltaic battery; G, G binding screws attached to the ends of the secondary wire. Slips of copper are attached to the bar D, so that when the bar rotates the connexion of the mercury cups, E, E, with the primary coil, and consequently with the voltaic battery, is made and broken at every revolution. The weight of the iron wires is about 8 lbs., the primary coil about 4½ lbs., and of the secondary wire about 1 lb. The entire apparatus is mounted on a strong wooden base.

23. On making connexion with the primary wire, by the binding screws F, F, to a series of twenty of the constant battery large cells (17.), the bundle of iron wires

becomes intensely magnetic, and the bar D rotates with great rapidity. Copper wires attached to the secondary coil by the screws G, G, are connected with the pillars of the micrometer electrometer (12.); and when the platinum points or plates of this instrument are approximated to $\frac{1}{100}$th of an inch, *a minute but brilliant spark appears* at every revolution of the bar.

24. When the pillars of the micrometer electrometer (12.) were separated three or four inches, the spark could be taken by the knuckle, or by a metallic ball, from either of the balls attached to the pillars, which then became the terminations of the secondary coil; and a Leyden jar was charged sufficient to deflect the leaves of a gold-leaf electroscope.

25. On introducing an extremely delicate Harris's thermo-electrometer in the current of the secondary wire, the liquid rose 3°.

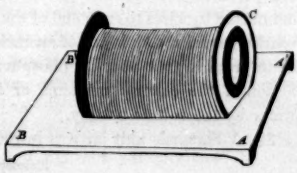
26. A voltameter filled with water slightly acidulated with sulphuric acid, was introduced in the secondary circuit, and the gases were evolved in minute bubbles as the bar rotated; and when charcoal points were attached to the terminations of the secondary coil, and these were approximated by means of the micrometer electrometer (12.) to within $\frac{1}{500}$th of an inch, the sparks appeared in such quick succession while the armature rotated, as to have the appearance of a continued but glimmering flame.

27. Thin paper introduced between the platinum or charcoal terminals, was perforated at every revolution of the bar, *but no increase of distance in the spark could be obtained through the vacuum obtained by means of a good air-pump.*

28. In the preceding experiments I had obtained most of the usual results, which are generally denominated intensity effects: the spark (23.); charge of a Leyden jar (24.); heating power (25.); chemical action (26.). In respect to the shock, it was most intense. One revolution of the bar, when the hands were in contact with the terminations of the secondary wire, created a most painful sensation. On two occasions I inadvertently touched both wires, the armature making three or four revolutions; the pain was so intense that I suffered from the effects for a considerable time afterwards; but as this effect, as is well known to all who have experimented with the secondary coil, is so much increased by the introduction of iron wires in the helix, I repeated the experiments under rather different circumstances, viz. by avoiding the use of this metal.

29. For this purpose the apparatus (22.) was removed, and a helix composed of 150 feet of No. 18 copper wire, covered with cotton, formed the primary, and 2100 feet of No. 26, similarly insulated, formed the secondary, the whole wound round a hollow cylinder of wood. Fig. 4. represents the form of the coil; C the coil, the shaded part representing the primary, and the

Fig. 4.



outer circle, the secondary wires; at A, A′ are binding screws, by which the connexions may be made with the primary wire to the battery (17.), and at B, B′ similar screws attached to the terminations of the secondary.

30. The primary coil (29.) being introduced in the circuit of a battery of twenty of the large cells (17.), and connexion made from the secondary coil to the pillars of the micrometer electrometer (12.), whenever the connexion with the primary wire was *broken*, a spark passed through a space of $\frac{3}{5000}$ths of an inch; but no effect could be observed on the liquid in Harris's thermo-electrometer (25.), until a bundle of iron wires was introduced in the centre of the helix, when the liquid rose 3°.

31. When two galvanometers are introduced, one in the circuit of the primary and the other in that of the secondary wire, the deflections of the needles will indicate currents in opposite directions. This action was noticed by Dr. Faraday in some of his earliest Researches*. In the course of these experiments I however repeatedly observed that no effect could be obtained on an exceedingly delicate galvanometer introduced in the *circuit* of the secondary wire, *unless it was completed by actual contact*. I introduced two galvanometers, one A, in the circuit of the primary, and the other B, in that of the secondary; when the ends of the latter were approximated to within $\frac{1}{5000}$th of an inch, the spark appeared whenever the contact with the primary was broken, but without producing the slightest action on the galvanometer B, the needle A of course being acted on in obedience to the direction of the current.

32. Although I had succeeded in obtaining a distinct spark through an appreciable and *measured* space (16. 23. 24. 26. 30. 31.), still the effects could only be assumed as due to some secondary cause; for while with 100 series of Professor Daniell's powerful constant battery the spark could not pass through a space occupied by a single thickness of a silk handkerchief, yet with the apparatus (22.), a single pair, as I afterwards found, was sufficient to produce the spark.

33. In the next series of experiments, I used an apparatus of a very different description to those I have described, one which, although well known to every electrician, has been very little used in this country; I allude to the pile of Zamboni. The one I have consists of a 10,000 series of laminated zinc, paper, and oxide of manganese, each disc being about one inch in diameter: this pile was divided into ten separate piles of 1000 series each.

34. With this apparatus, using the micrometer electrometer, I obtained distinct sparks through a space of $\frac{1}{20}$th of an inch, when the two *balls* were used, one being connected by wires to each end of the pile; and when *points* were fixed to the pillars the sparks passed through $\frac{1}{10}$th of an inch. At $\frac{1}{50}$th the stream of sparks was so powerful as to produce that peculiar phosphorescent odour which is always perceptible in the action of the electric machine, or in the gases evolved from the decomposition of water by the voltaic battery.

35. A Harris's unit jar was introduced in the circuit, a wire from one end of the

* Philosophical Transactions, 1831.

pile being attached to the outer, and one from the other to the inner coating. On the two small balls which are fixed to this really beautiful and simple instrument being approximated to about $\frac{1}{50}$th of an inch, the jar was charged and discharged every three seconds, and this action continued for several minutes without the slightest variation.

36. I have since repeated the experiment with one pile or 1000 series: a spark passed between the *plate* and *point* of the micrometer electrometer through a space of $\frac{19}{1000}$ths of an inch, but I could not obtain any effect, even with the entire series, on HARRIS's thermo-electrometer, or with a delicate galvanometer, or any evidence of chemical action*.

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37. Subsequently to the completion of the experiments detailed in this paper, my attention was drawn to a paper in the American Philosophical Transactions, entitled Contributions to Electricity and Magnetism, by Dr. HENRY of Philadelphia, Third Series. I have not had the opportunity of seeing the previous series, but find that the present investigations of this celebrated experimentalist have been directed to the discovery of inductive actions in common electricity analogous to those of galvanic, and I was in great hopes that Dr. HENRY had examined the action of the spark; his views, however, appear to me to have been directed to other objects.

In the twenty-ninth paragraph he alludes to the spark appearing when the ends of the second coil were rubbed together; in the thirty-first, that by uniting a number of coils the brilliancy of the spark was much reduced; and in the ninety-sixth, to the fact of the induced current in an adjoining conductor being more powerful than in the first, and that to render the spark visible, the electricity must be projected through a small distance of air: the following experiment was suggested by those of Dr. HENRY.

38. Five cells of Professor DANIELL's constant battery were charged and connected with the electro-magnetic machine (22.); the primary coil of the apparatus (29.) was connected with the secondary wire of the machine (22.), and the micrometer electrometer (12.) to the secondary wire or coil of the apparatus (29.); the armature of the machine rotated, but no spark appeared even through $\frac{1}{5000}$th of an inch, although slight shocks could be perceived; but when the *secondary* coils of both instruments (22. 29.) were connected together, a minute but brilliant spark passed at the break of the primary wire of the apparatus (29.) through $\frac{1}{5000}$th of an inch.

It is not my intention to intrude any theoretical opinions of my own, but I will

* I have since repeated the experiments as to the evidence of chemical action with great care, and after many trials have at length been enabled to obtain chemical decomposition of a solution of iodide of potassium in the following manner. I fastened about two inches of platinum wire to each end of the pile of 10,000 series; the two points were approximated parallel to each other, about a quarter of an inch apart; a piece of bibulous paper, saturated with a solution of iodide of potassium, placed on a slip of glass, was then brought into contact with the ends of the wires, and the iodine invariably appeared on that attached to the end of the pile terminating with the oxide of manganese.—*Nov. 19th*, 1839.

content myself by briefly referring to those which are now generally received as axioms in electrical science.

The effects that are obtained from different sources of electricity, as well as those obtained from different forms and dimensions of the voltaic battery, have been conveniently generalized under the terms QUANTITY and INTENSITY. Let us now examine the results obtained by the experiments detailed in this paper; a series of one hundred of Professor DANIELL's constant battery, excited with strong acid solutions, will scarcely affect the leaves of a delicate gold-leaf electroscope, yet a single excitation by a silk handkerchief on the glass globe by which they are surrounded, will rend the leaves of such an instrument to shreds; and we find that the space occupied by a single fold of a silk handkerchief, or even a piece of tissue paper, is sufficient to insulate a power, which after the circuit has been once completed will ignite upwards of sixteen feet of thick platinum wire and fuse titanium into a solid mass, effects never, I believe, before obtained by the voltaic battery.

A one thousand series of ZAMBONI's piles is sufficient to elicit a distinct spark to the measured space of $\frac{19}{1000}$ths of an inch; yet these different results are at once assumed as being due to what is usually denominated quantity and intensity.

It has been often urged that any objection which may have arisen as to the identity of electricity and galvanism, was at once removed by the fact of a spark of the latter being capable of passing through space, or what is usually denominated the striking distance; and it argues little for the correctness of our experimental investigations, that such an important fact, (although perhaps until it is more satisfactorily proved it can scarcely be assumed as a fact,) one apparently so easy of confirmation, should rest on one or two doubtful and isolated experiments. Although I must consider that Professor DANIELL laboured under some error when he describes the discharge passing in the form of a spark (8.) when the cells were approximated, yet I cannot but feel that it will be with the aid, and through the principles of this philosopher's scientific apparatus, which he has so appropriately denominated the constant battery, that the true principles of voltaic action will be correctly ascertained. There is already one fact which was obtained with the 160 pairs (10.) which cannot, I believe, be satisfactorily accounted for by any of the existing theories. I allude to the remarkable heating of the positive electrode two inches *beyond* the part where the circuit was completed*.

Before I conclude, I should wish it to be distinctly understood that I offer no opinion as to the possibility of obtaining the striking distance or spark before contact; I merely present an account of those experiments which have been made by me to obtain it. That according to the present theoretical views of the action of the voltaic battery, with the apparatus I used, it ought to have taken place, I think most persons will acknowledge; and also, that if by still more powerful apparatus it cannot be produced, the theory must in some way or other be incorrect.

* Transactions of the Electrical Society, Part I. p. 65.

X. *Researches in Physical Geology.—Second Series.* *By* W. Hopkins, *Esq. M.A. F.R.S., Fellow of the Royal Astronomical Society, of the Geological Society, and of the Cambridge Philosophical Society.*

Received February 17,—Read March 7, 1839.

On Precession and Nutation, assuming the Interior of the Earth to be fluid and heterogeneous.

HAVING in my last memoir completed the investigation of the amount of Precession and Nutation on the hypothesis of the earth's consisting of a homogeneous fluid mass contained in a homogeneous solid shell, I shall now extend the investigation to the case in which both the interior fluid and exterior shell are considered heterogeneous.

1. When accelerating forces X, Y, Z act on any point x, y, z of a heterogeneous fluid mass, of which no part of the surface is free, and of which the density at the point x, y, z is ϱ, we have for the conditions of equilibrium

$$\text{(I.)} \qquad X\left(\frac{dY}{dz}-\frac{dZ}{dy}\right)+Y\left(\frac{dZ}{dx}-\frac{dX}{dz}\right)+Z\left(\frac{dX}{dy}-\frac{dY}{dx}\right)=0.$$

(II.) ϱ = constant throughout each surface of equal pressure.

Now the forces which act on the internal fluid of the earth are

(1.) The mutual attraction of the different particles of the fluid mass:

(2.) The attraction of the solid shell on the fluid mass:

(3.) The disturbing force of the sun:

(4.) The disturbing force of the moon:

The centrifugal force, the planes of rotation being parallel to the tangent plane at B′ (fig. 2. First Series, Art. 8.): This may be separated into two parts, viz.

(5.) The resolved part on any point acting in a direction perpendicular to the axis of rotation; and

(6.) The resolved part parallel to the axis of rotation.

The forces (1.) and (2.) satisfy condition (I.), as is well known; as do likewise (3.), (4.) and (5.), (First Series, Arts. 8, 9, 10.). The force (6.), as has been shown (First Series, Art. 10.), gives $Z = 2\,\omega^2\,\epsilon\,\beta\,.\,x$, which does not really satisfy the condition expressed by equation (I.), since it does not satisfy the well-known equations from which it is derived*. If, therefore, the forces (1.) (2.) (3.) (4.) and (5.) only acted on

* It is hardly necessary to remark that equation (I.) is satisfied by $Z = 2\,\omega^2\,\epsilon\,\beta\,.\,x$, only in consequence of the introduction of the extraneous factor Y (which here = 0) in the elimination of ρ from the three equations from which (I.) is derived.

the fluid mass it would be in equilibrium, provided condition (II.) were satisfied; and if in addition to these the force (6.) should also act, it would be the only force producing motion in the fluid, provided the instantaneous surface of equal density should always be identical with that which would be the surface of equal pressure if the force (6.) did not act, since such an arrangement of the fluid would be necessary in order that the forces (1.) (2.) (3.) (4.) (5.) should produce no motion in the fluid.

It has been shown (First Series, Art. 12.) that the force $Z\ (= 2\omega^2 \varepsilon \beta . x)$ on any particle $(x\ y\ z)$ may be replaced by a force $\omega^2 \varepsilon \beta . r$, acting perpendicularly to the radius vector, and producing motion in the fluid, and another force which would be consistent with the equilibrium of the fluid. The latter of these may therefore be associated with the above forces, which would produce equilibrium, and the former $(\omega^2 \varepsilon \beta . r)$ will be the only force producing motion, always supposing condition (II.) to be satisfied with reference to all those forces which satisfy condition (I.). Consequently if these forces were such that the surfaces of equal density should be spherical, and if the inner surface of the shell were also accurately spherical, the angular velocity generated in the fluid about the axis of y in a unit of time would $= \omega^2 \varepsilon \beta$, and would be entirely independent of the law according to which the density should vary in passing from one spherical surface of equal density to another. It would be the same for the heterogeneous as for the homogeneous fluid.

The action of the force $\omega^2 \varepsilon \beta . r$, and therefore the above result will be modified in the case of the earth by the circumstances of the inner surface of the shell and the surfaces of equal density being spheroidal, instead of being spherical. It is manifest, however, that this modification will be of an order higher than $\omega^2 . \varepsilon \beta$, and may therefore be neglected, as before stated with reference to the homogeneous spheroid (First Series, Art. 14.).

Again, in consequence of the fluid motion produced by the force $\omega^2 \varepsilon \beta . r$, the condition (II.) will not be accurately satisfied with reference to the forces which satisfy (I.), and these forces will therefore produce motion in the fluid. To estimate this motion, let us suppose no forces to act on the fluid except those which satisfy (I.), and let us suppose the surfaces of equal density to be displaced through indefinitely small spaces from those positions in which there would be perfect equilibrium. The tendency of the forces would be to bring back the instantaneous surfaces of equal density to their latter positions, and thus an oscillatory motion would arise; but it is manifest that no continuous angular motion of the whole mass could thus be produced, at least of the same order of magnitude as the whole oscillatory motion. Now, in our actual case, this motion could not be greater than quantities of the order $\varepsilon \beta$, since the angular motion from which the perturbation we are considering arises, is of that order. Consequently the modification of our first result due to this cause may be neglected.

Again, the disturbing forces of the sun and moon will produce an oscillation in the surfaces of equal density of the fluid mass, or an internal tide; but it is manifest that

no continuous angular motion of the whole fluid mass can arise from this oscillatory motion, which may here, therefore, be also neglected.

Hence, then, the angular motion of the fluid about the axis of y will be the same, to the required degree of approximation, whether the earth be heterogeneous or homogeneous; i. e. if, for the heterogeneous spheroid, we denote by (γ_2) the quantity analogous to that in the homogeneous one denoted by γ_2 (First Series, Art. 16.), we shall have

$$(\gamma_2) = \gamma_2.$$

§. *Motion of the Shell.*

I shall now proceed to determine the motion of the heterogeneous shell; for which purpose we must find the values which the quantities $A_1\ B_1\ D_1\ A_2\ B_2\ D_2$, &c. (First Series, Art. 5.) assume when the spheroid is heterogeneous; or, adopting the notation of the last paragraph, we must find the values of (A_1) (A_2) (A_3) (A_4) (B_1) (B_2) (B_3) (B_4) (D_1) (D_2) (D_3) (D_4), and (γ_1).

2. The moment of the disturbing force of the sun communicating a rotatory motion to the earth, considered as a heterogeneous spheroid*,

$$= \frac{3\mu}{2r_1^3} \sin 2\Delta \frac{8\pi}{15} \int_0^{a_1} \varrho' \frac{d(a'^5 \epsilon')}{da'}\, da',$$

where a_1 = polar radius of the earth,

ϵ' = the ellipticity of that surface of equal density (ϱ') of which the polar radius is a'.

Consequently, if a be the polar radius of the inner surface of the shell, this moment for the shell will be

$$= \frac{3\mu}{2r_1^3} \sin 2\Delta \frac{8\pi}{15} \int_a^{a_1} \varrho' \frac{d(a'^5 \epsilon')}{da'}\, da'.$$

Also the moment of inertia of the shell

$$= \frac{8\pi}{15} \int_a^{a_1} \varrho' \frac{d(a'^5)}{da'}\, da'.$$

And hence we have (First Series, Art. 19.)

$$\frac{(\alpha)}{\omega} = \frac{3}{2\omega} \cdot \frac{\mu}{r_1^3} \frac{\int_a^{a_1} \varrho' \frac{d(a'^5 \epsilon')}{da'}\, da'}{\int_a^{a_1} \varrho' \frac{d(a'^5)}{da'}\, da'} \sin 2\Delta,$$

$$= \frac{3\pi}{\nu T} \cdot \frac{\int_a^{a_1} \varrho' \frac{d(a'^5 \epsilon')}{da'}\, da'}{\int_a^{a_1} \varrho' \frac{d(a'^5)}{da'}\, da'} \sin 2\Delta.$$

* Airy's Tracts, p. 207., Second Edition.

Let ϵ be the ellipticity of the inner surface of the shell. If the earth were homogeneous, and the ellipticity of the external surface were also $= \epsilon$, we should have

$$\frac{\alpha}{\varpi} = \frac{3\pi}{\nu T}\epsilon \sin 2\Delta \text{ (First Series, Art. 19.)};$$

and therefore

$$\frac{(\alpha)}{\varpi} = \frac{\int_a^{a_1} \rho' \frac{d(a'^5\epsilon')}{da'}\, da'}{\int_a^{a_1} \rho' \frac{da'^5}{da'}\, da'} \cdot \frac{1}{\epsilon} \cdot \frac{\alpha}{\varpi}.$$

Let us suppose this

$$= (1+s)\frac{\alpha}{\varpi};$$

then

$$s = \frac{\int_a^{a_1} \rho' \frac{d(a'^5\epsilon')}{da'}\, da'}{\int_a^{a_1} \rho' \frac{da'^5}{da'}\, da'} \cdot \frac{1}{\epsilon} - 1,$$

which for brevity may be written

$$s = \frac{k}{\epsilon} - 1.$$

If ϵ' were constant and $= \epsilon_1$, we should have

$$s = \frac{\epsilon_1}{\epsilon} - 1.$$

In our actual case we may put

$$s = \eta\frac{\epsilon_1}{\epsilon} - 1;$$

we shall then have

$$\eta = \frac{k-\epsilon}{\epsilon_1 - \epsilon}.$$

Unless $a_1 - a$ be very small it is manifest that k will be less than ϵ_1, and therefore η less than unity.

The value of $\frac{(\alpha)}{\varpi}$ gives

$$(A_1) = (1+s)A_1 \qquad \text{(First Series, Art. 19.)}.$$

Similarly

$$(B_1) = (1+s)B_1,$$
$$(D_1) = (1+s)D_1,$$
$$(A_2) = (1+s)A_2,$$
$$(B_2) = (1+s)B_2,$$
$$(D_2) = (1+s)D_2.$$

3. The fluid pressure on the interior surface of the shell will be produced by the mutual attractions of the particles of the whole mass fluid and solid, the centrifugal

force on the fluid, and the disturbing forces of the sun and moon. If p denote the fluid pressure at any point x, y, z, and ϱ the density there, we have

$$\frac{dp}{\varrho} = \left(X - \frac{d^2x}{dt^2}\right)dx + \left(Y - \frac{d^2y}{dt^2}\right)dy + \left(Z - \frac{d^2z}{dt^2}\right)dz.$$

The accurate determination of the values of $\frac{d^2x}{dt^2}$, $\frac{d^2y}{dt^2}$ and $\frac{d^2z}{dt^2}$, would require that of the motion of the fluid; but since these quantities will be very small, it will be sufficient to determine their approximate values. The angular motion denoted by α' (First Series, Art. 15.) may be neglected in determining the value of p, as shown in Art. 23. (First Series). The motion to be taken account of is the internal tidal oscillation. The direction of each particle's tidal motion will be approximately in a line through the earth's centre; and therefore, if we suppose a portion of the fluid to be contained in a rectilinear and rigid canal of small diameter, passing through the centre of the earth, the motion of the fluid and the fluid pressure at any point within the canal will not be affected. Let r be the distance of an element of the fluid contained in this canal from the centre of the earth; R the sum of the impressed forces acting on this element resolved in the direction of the canal; then, instead of the general equation above, we shall have

$$dp = \varrho\left(R - \frac{d^2r}{dt^2}\right)dr,$$

and

$$p = \Pi + \int_0^{\bar{r}} \varrho\left(R - \frac{d^2r}{dt^2}\right)dr,$$

Π being the pressure at the centre, $\bar{r}$ the value of r at the interior surface of the shell, and p the pressure there; and we shall have the moment of p round the axis of y (First Series, Art. 21.)

$$= 2\varepsilon\Sigma x\delta S\cos\zeta\left(\Pi + \int_0^{\bar{r}} \varrho\left(R - \frac{d^2r}{dt^2}\right)dr\right).$$

The value of R is given by the equation

$$R = X\cos\theta'' + Y\cos\theta' + Z\cos\theta,$$

θ, θ', θ'' being the angles which the direction of the canal makes with the axes of z, y, and x respectively. We may consider separately the effects of the different forces above mentioned which combined produce the force R.

4. Let us first take that part of R which depends on the mutual attractions of the particles constituting both the fluid and solid portions of the mass.

If we examine the part of the expression under the sign Σ for the moment about the axis of y, given in article 23 of my first memoir, we observe that it consists partly of terms involving β as a factor, and partly of terms independent of β, but that all the latter disappear when the integration is performed between the proper limits, leaving a result containing β as a factor. It was easy to foresee that this must be the case,

because the moment considered in the article referred to was that of the centrifugal force, and would manifestly vanish with β. In like manner it is manifest that the moment about the axis of y, produced by that part of R which depends on the mutual attractions of the particles of the whole mass, must vanish with β, i. e. when the instantaneous axis of rotation of the fluid coincides with the spheroidal axis of the shell. Consequently all the terms which need be retained in R, in the above expression, must involve β as a factor, and, therefore, all terms in R involving ϵ may be neglected; for since the terms retained must involve β, those involving ϵ would be of the order $\epsilon\beta$ under the integral sign; and since the whole integral is multiplied by ϵ, the corresponding terms in the result would be of the order $\epsilon^2\beta$, terms which have always been rejected. With this restriction the attraction on a point at the distance r from the centre in the direction of r, or the value of R, will

$$= \frac{4\pi \int_0^r \varrho' r'^2 dr'}{r^2},$$

$$= \frac{4\pi \Phi(r)}{r^2};$$

and the corresponding moment about the axis of y

$$= 2\epsilon \Sigma x\delta \mathrm{S} \cos\zeta \left(\Pi + 4\pi \int_0^{\bar{r}} \varrho \frac{\Phi(r)}{r^2} dr\right).$$

Again, in the expression for ϱ we may reject all terms involving ϵ, for the same reason as they have been rejected in the expression for R, which will reduce the expression for ϱ to the same form as if the surface of equal pressure were a sphere, i. e. ϱ will become a function of r. Hence, since the quantity under the sign $\int$ will now be a function of r, and all terms in the definite integral which do not involve the factor β must disappear, the above expression for the moment about the axis of y may be written

$$2\epsilon \Sigma x\delta \mathrm{S} \cos\zeta (\Pi + \beta \Psi(\bar{r})).$$

Also (a being, as heretofore, the axis of the interior surface of the shell)

$$\bar{r} = a + \text{terms involving } \epsilon;$$

and therefore for $\Psi(\bar{r})$ we may substitute $\Psi(a)$; and the moment becomes

$$= 2\epsilon (\Pi + \beta \Psi(a)) \Sigma x\delta \mathrm{S} \cos\zeta$$

$$= 0$$

when integrated between the proper limits.

5. Let us now consider that part of R which depends on the centrifugal force on the fluid. Taking those terms in the expression for the resolved parts of this force which are independent of the factor $\epsilon\beta$ (First Series, Art. 23.), we have for these parts $\omega^2 x'$ and $\omega^2 y'$, parallel to the axes of x' and y' respectively, the axis of rotation

of the fluid being the axis of z'. Making the spheroidal axis of the shell the axis of z, and the plane of $x\,z$ the same as that of $x'\,z'$, we have

$$\begin{aligned} x' &= x\cos\beta + z\sin\beta \\ &= x + z\sin\beta, \\ y' &= y. \end{aligned}$$

Therefore

$$\begin{aligned} \mathrm{X} &= \omega^2 x'\cos\beta = \omega^2\{x\cos^2\beta + z\sin\beta\cos\beta\}, \\ \mathrm{Y} &= \omega^2 y' \qquad = \omega^2 y, \\ \mathrm{Z} &= \omega^2 x'\sin\beta = \omega^2\{x\cos\beta\sin\beta + z\sin^2\beta\}. \end{aligned}$$

Also

$$\begin{aligned} z &= r\cos\theta, \\ y &= r\sin\theta\sin\varphi, \\ x &= r\sin\theta\cos\varphi; \end{aligned}$$

and therefore

$$\cos\theta' = \frac{x}{r} = \sin\theta\cos\varphi,$$

$$\cos\theta' = \frac{y}{r} = \sin\theta\sin\varphi.$$

Hence

$$\mathrm{R} = \mathrm{X}\cos\theta' + \mathrm{Y}\cos\theta' + \mathrm{Z}\cos\theta,$$

$$= \omega^2\left\{\left(x\cos^2\beta + \frac{z}{2}\sin 2\beta\right)\sin\theta\cos\varphi + \omega^2 y\sin\theta\sin\varphi + \left(\frac{x}{2}\sin 2\beta + z\sin^2\beta\right)\cos\theta\right\}$$

In this expression all the terms may be omitted except those which involve β as a factor, for the reasons assigned above (Art. 4.), and thus (omitting also terms of the order β^2) the above expression becomes

$$\frac{\omega^2}{2}\sin 2\beta\,(z\sin\theta\cos\varphi + x\cos\theta),$$

$$= \omega^2\sin 2\beta\sin\theta\cos\theta\cos\varphi\, r.$$

And

$$\int_0^{\bar r}\varrho\,\mathrm{R}\,dr = \omega^2\sin 2\beta\sin\theta\cos\theta\cos\varphi\int_0^{\bar r}\varrho\, r\,dr.$$

Or putting $\bar r = a$, we have the moment about the axis of y

$$= 2\varepsilon\Sigma x\delta\mathrm{S}\cos\zeta\left(\Pi + \omega^2\sin 2\beta\sin\theta\cos\theta\cos\varphi\int_0^a\varrho\, r\,dr\right).$$

But

$$x\delta\mathrm{S}\,.\cos\zeta = a^3\sin^2\theta\cos\theta\cos\varphi\,\delta\varphi\,\delta\theta. \qquad \text{(First Series, Art. 23.)}$$

and therefore, putting

$$\int_0^a\varrho\, r\,dr = f(a),$$

and omitting the first term in the above expression (since it vanishes between the

proper limits), we have the moment

$$= 2\,\omega^2\,\varepsilon \sin 2\beta\, a^3 f(a) \iint \sin^3\theta \cos^2\theta \cos^2\varphi\, d\varphi\, d\varphi\, d\theta,$$

$$= \frac{2 f(a)}{a^3}\,\omega^2\,\varepsilon \sin 2\beta\, a^5 \iint \sin^3\theta \cos^2\theta \cos^2\varphi\, d\varphi\, d\varphi\, d\theta,$$

$$= \frac{2 f(a)}{a^3} \cdot \frac{4\pi}{15}\,\omega^2\,\varepsilon \sin 2\beta\, a^5.$$

If ϱ be constant, $2f(a) = a^2$; and this expression becomes the same as that previously obtained (First Series, Art. 23.) for the homogeneous spheroid.

The moment of inertia of the shell

$$= \frac{8\pi}{15} \int_a^{a_1} \varrho\, \frac{dr^5}{dr}\, dr,$$

(omitting terms involving ε)

$$= \frac{8\pi}{15} \left\{ \sigma(a_1) - \sigma(a) \right\},$$

where

$$\sigma(r) = \int_0^r \varrho\, \frac{dr^5}{dr}\, dr.$$

Hence the accelerating force of rotation on the shell produced by the part of R now considered

$$= (\alpha'') = \frac{2 f(a)\, a^3}{\sigma(a_1) - \sigma(a)} \cdot \frac{\omega^2}{2}\,\varepsilon \sin 2\beta,$$

$$= \frac{2 f(a)\, a^3 (q^5 - 1)}{\sigma(a_1) - \sigma(a)} \cdot \frac{\omega^2 \varepsilon}{2(q^5 - 1)} \sin 2\beta;$$

or if

$$\frac{2 f(a)\, a^3 (q^5 - 1)}{\sigma(a_1) - \sigma(a)} = h,$$

$$\frac{(\alpha'')}{\omega} = h\, \frac{\omega \varepsilon}{2(q^5 - 1)} \sin 2\beta,$$

$$= h\, \gamma_1 \sin 2\beta, \qquad \text{(First Series, Art. 23.)}$$

which gives

$$(\gamma_1) = h\, \gamma_1$$

6. We have now to consider that part of R which depends on the sun's action.

Let X', Y', Z' be the disturbing forces of the sun parallel to the axes of x, y, z respectively, the plane of $x\,z$ being now so taken as to pass through the sun's centre. Then if Δ be as heretofore the N. P. D. of the sun, we have (First Series, Art. 21.)

$$X' = \frac{2\mu}{r_1^3}\, x' \sin\Delta + \frac{\mu}{r_1^3}\, z' \cos\Delta,$$

$$Y' = -\frac{\mu}{r_1^3}\, y',$$

$$Z' = \frac{2\mu}{r_1^3}\, x' \cos\Delta - \frac{\mu}{r_1^3}\, z' \sin\Delta,$$

where

$$x' = x \sin \Delta + z \cos \Delta,$$
$$y' = y,$$
$$z' = z \sin \Delta - x \cos \Delta;$$

and therefore, by substitution,

$$X' = \frac{\mu}{r_1^3}\left\{\left(\frac{1}{2} - \frac{3}{2}\cos 2\Delta\right)x + \frac{3}{2}\sin 2\Delta \,.\, z\right\},$$

$$Y' = -\frac{\mu}{r_1^3} y,$$

$$Z' = \frac{\mu}{r_1^3}\left\{\left(\frac{1}{2} + \frac{3}{2}\cos 2\Delta\right)z + \frac{3}{2}\sin 2\Delta \,.\, x\right\}.$$

In the formula

$$R = X \cos \theta'' + Y \cos \theta' + Z \cos \theta,$$

putting for X, Y, Z the above values of X', Y', Z', we find

$R = \frac{3\mu}{r_1^3} \sin 2\Delta \cos \theta \sin \theta \cos \varphi \, r +$ terms which involve even powers of $\cos \varphi$, and which, therefore, when multiplied by $x\,\delta\,S \cos \zeta$ $(= a^3 \sin^2 \theta \cos \theta \cos \varphi \, d\varphi \, d\theta)$, will contain odd powers of $\cos \varphi$, and will therefore disappear after the final integration. Hence then the moment about our present axis of y

$$= 2\,s\,\Sigma\, x\,\delta\, S \cos \zeta \left(\Pi + 3\,\frac{\mu}{r_1^3} \sin 2\Delta \sin \theta \cos \theta \cos \varphi \int_0^a \rho\, r\, dr\right),$$

$$= 6\,\frac{\mu}{r_1^3}\, s \sin 2\Delta\, a^3 f(a) \iint \sin^3 \theta \cos^2 \theta \cos^2 \varphi \, d\theta \, d\varphi,$$

$$= \frac{2f(a)}{a^2} \cdot \frac{4\pi}{5}\,\frac{\mu}{r_1^3}\, s \sin 2\Delta \,.\, a^2 \quad \text{(Art. 5.)}.$$

If ρ be constant and $=$ unity, $2f(a) = a^2$, and the expression becomes the same as that for the homogeneous spheroid.

Dividing the above quantity by the moment of inertia of the shell, we have the angular accelerating force

$$= (\alpha') = \frac{2f(a)\,a^2}{\sigma(a_1) - \sigma(a)} \cdot \frac{3}{2} \cdot \frac{\mu}{r_1^3}\, s \sin 2\Delta,$$

$$= h\,\frac{3}{2} \cdot \frac{\mu}{r_1^3} \cdot \frac{s}{q^5 - 1} \sin 2\Delta \qquad \text{(Art. 5.)},$$

$$= \frac{h}{q^5 - 1}\,\alpha' \qquad \text{(First Series, Art. 21.)};$$

and therefore

$$\frac{(\alpha')}{\omega} = \frac{h}{q^5 - 1} \cdot \frac{\alpha'}{\omega}.$$

Hence we have

$$(A_3) = h\, A_3;$$

and similarly

$$(B_3) = h\, B_3,$$
$$(D_3) = h\, D_3.$$

By a precisely similar investigation applied to the moon's action we obtain

$$(A_4) = h A_4,$$
$$(B_4) = h B_4,$$
$$(D_4) = h D_4.$$

7. It remains for us to determine the approximate value of $\frac{d^2 r}{dt^2}$.

Since the exact position and form of any surface of equal pressure and density in the internal fluid will depend on the disturbing forces of the sun and moon, as well as on the mutual attractions of the different particles of the whole mass, it will be constantly varying with the positions of those luminaries. The hypothesis I shall make for the purpose of determining an approximate value of $\frac{d^2 r}{dt^2}$ is this—that the instantaneous position and form of any surface of equal pressure and density are the same as those it would have in its position of equilibrium under the action of the forces at the proposed instant, and supposing the whole mass fluid. Each fluid particle would then move with the surface to which it belongs, and in a direction at least very approximately normal to it, and therefore passing very nearly through the centre of the earth, as previously stated (Art. 3.). The extreme slowness of the absolute motion of each particle will render this hypothesis quite approximate enough for our purpose.

We must first determine the surface of equal pressure, supposing it one of equilibrium; and in doing this we may restrict the investigation to the case of the sun's action. From the results thus obtained those depending on the moon's action will be immediately deducible.

Let X, Y, Z be the whole of the forces on the fluid particle (x, y, z) parallel to the coordinate axes, arising from the attractions of the other particles of the mass, the centrifugal force, and the disturbing force of the sun. Then

$$\frac{dp}{\varrho} = X\,dx + Y\,dy + Z\,dz;$$

and if $\frac{dV}{dx}$, $\frac{dV}{dy}$, and $\frac{dV}{dz}$ be the attractions on the particle x, y, z,

$$X = \frac{dV}{dx} + \omega^2 x + \frac{\mu}{r_1^3}\left\{\left(\tfrac{1}{2} - \tfrac{3}{2}\cos 2\Delta\right)x + \tfrac{3}{2}\sin 2\Delta \,.\, z\right\},$$
$$Y = \frac{dV}{dy} + \omega^2 y - \frac{\mu}{r_1^3} y,$$
$$Z = \frac{dV}{dz} + \frac{\mu}{r_1^3}\left\{\tfrac{3}{2}\sin 2\Delta \,.\, x + \left(\tfrac{1}{2} + \tfrac{3}{2}\cos 2\Delta\right)z\right\}.$$

substituting the values of X′, Y′ and Z′ given in the preceding article. Consequently we have for the surface of equal pressure

$$C = V + \tfrac{\omega^2}{2}(x^2 + y^2) + \frac{\mu}{2 r_1^3}\left\{\left(\tfrac{1}{2} - \tfrac{3}{2}\cos 2\Delta\right)x^2 - y^2 + \left(\tfrac{1}{2} + \tfrac{3}{2}\right)\cos 2\Delta \,.\, z^2 + 3\sin 2\Delta\, x\, z\right\}.$$

Putting

$$z = r\cos\theta,$$
$$y = r\sin\theta\sin\phi,$$
$$x = r\sin\theta\cos\phi,$$

and arranging the results in terms which severally satisfy the equation of LAPLACE's coefficients, we obtain

$$C = V + \frac{\omega^2}{3}a^2 + \left(\frac{\omega^2}{2} + \frac{3}{r_1^3}\frac{\mu}{r_1^3}\cdot\frac{1+3\cos 2\Delta}{8}\right)a^2\left(\frac{1}{3}-\gamma^2\right),$$

$$+\frac{3}{8}\frac{\mu}{r_1^3}(1-\cos 2\Delta)\,a^2(1-\gamma^2)\cos 2\phi + \frac{3}{2}\frac{\mu}{r_1^3}\sin 2\Delta\,.\,a^2\gamma\sqrt{1-\gamma^2}\cos\phi,$$

where $\gamma = \cos\theta$, and a is a mean value of r for any surface of equal density. Or transposing the constant term, and putting $a^2\,F(\theta\phi)$ for the sum of the other small terms,

$$\text{Const.} = V + a^2\,F(\theta\phi).$$

Putting $r = a(1+\alpha y)$, where α is a small coefficient, and y here denotes a function of θ and ϕ, we have

$$V = \frac{4\pi}{a}(1-\alpha y)\int_0^a \rho'\left\{a'^2 + \alpha\frac{d}{da'}\left(\frac{a'^4}{3a}Y_1' + \ldots + \frac{a'^{i+3}}{(2i+1)a^i}Y_i' + \&c.\right)\right\}da'$$
$$+ 4\pi\int_a^{a_1}\rho'\left\{a' + \alpha\frac{d}{da'}\left(\frac{a\,a'}{3}Y_1' + \ldots + \frac{a^i}{(2i+1)a'^{i-2}}Y_i' + \&c.\right)\right\}da',$$

a being so taken that $Y_0 = 0$, and a_1 = value of a at the earth's external surface. Putting $y = Y_1 + Y_2 +$, &c., and substituting the resulting value of V in the above equation, we shall have $Y_i = 0$, except $i = 2$; and we shall have for the determination of Y_2 the equation

$$0 = \frac{4\pi}{a}\alpha\int_0^a\rho'\frac{d}{da'}\left(\frac{a'^5}{5a^3}Y'_2\right)da' - \frac{4\pi}{a}\alpha\,Y_2\int_0^a\rho'a'^2\,da'$$
$$+ 4\pi\alpha\int_a^{a_1}\rho'\frac{d}{da'}\left(\frac{a^2}{5}Y'_2\right)da' + a^2\,F(\theta\phi).$$

Y'_2 will differ from Y_2 only by small quantities, and therefore in determining the influence of the smaller terms in $F(\theta\phi)$ $\left(\text{those which involve }\frac{\mu}{r_1^3}\right)$ on the value of Y_2 we may consider $Y'_2 = Y_2$, and we then obtain

$$\alpha\,Y_2 = \frac{a^2\,F(\theta\phi)}{\frac{4\pi}{a}\int_0^a\rho'a'^2\,da' - \frac{4\pi}{a^3}\int_0^a\rho'a'^4\,da'}.$$

Hence

$$r = a(1+\alpha\,Y_2) = a\left\{1 + \frac{a^2\,F(\theta\phi)}{\frac{4\pi}{a}\int_0^a\rho'a'^2\,da' - \frac{4\pi}{a^3}\int_0^a\rho'a'^4\,da'}\right\}.$$

If g' denote the force of gravity at the distance a from the centre of the earth, we have

$$\frac{4\pi}{a}\int_0^a\rho'a'^2\,da' = g'a.$$

To find the value of the second term of the denominator in the above value of r, I shall take the well-known expression for the density, viz.

$$\rho' = A \frac{\sin q' a'}{a'},$$

where $q' a_1 = 150°$. This gives us

$$\int_0^a \rho' a'^2 \, d a' = \frac{A}{q'^3 a^3} (\sin q' a - q' a \cos q' a);$$

whence

$$\frac{A}{q'^3 a^3} (\sin q' a - q' a \cos q' a) = g' \frac{a^2}{4\pi}.$$

Also

$$\int_0^a \rho' a'^4 \, d a' = \frac{A}{q'^4 a^4} \left\{ (6 q' a - q'^3 a^3) \cos q' a + (3 q'^2 a^2 - 6) \sin q' a \right\};$$

and substituting the value of A from the preceding equation,

$$\frac{4\pi}{a^3} \int_0^a \rho' a'^4 \, d a' = \frac{(6 q' a - q'^3 a^3) \cos q' a + (3 q'^2 a^2 - 6) \sin q' a}{q'^3 a^3 (\sin q' a - q' a \cos q' a)} g' a.$$

The numerical value of the multiplier of $g' a$ in this expression is nearly constant for different values of a, from $a = 0$ to $a = a_1$, being $\frac{3}{5}$ in the former case and $\frac{1}{2}$ (very nearly) in the latter. If we take the mean of these values for that of the quantity in question,

$$\frac{4\pi}{a^3} \int_0^a \rho' a'^4 \, d a' = \frac{11}{20} g' a \text{ very nearly.}$$

Hence we have

$$r = a \left\{ 1 + \frac{20}{9} \cdot \frac{a}{g'} F(\theta \varphi) \right\};$$

and if g be the value of g' at the earth's surface where $a = a_1$

$$\frac{a}{g'} = \frac{a_1}{g} \text{ approximately;}$$

and therefore

$$r = a \left\{ 1 + \frac{20}{9} \cdot \frac{a_1}{g} F(\theta \varphi) \right\},$$

the equation to any instantaneous surface of equal pressure, supposing its actual instantaneous and statical forms and positions to be the same. It is a prolate spheroid, of which the axis is in the line joining the centres of the earth and sun. This surface may be considered as fixed while the earth revolves in its diurnal motion, during which each fluid particle (according to our approximate hypothesis of its motion) will always remain in the same surface of equal pressure; and if r, θ and φ be taken as the coordinates of any one particle during its diurnal motion, θ (its angular distance from the pole of the earth) will remain constant, and $\frac{d\varphi}{dt}$ will equal the angular velocity of rotation. The corresponding variation in r will be obtained by

differentiating the previous equation subject to these conditions. Hence

$$\frac{d^2 r}{dt^2} = a\frac{20}{9}\cdot\frac{a_1}{g}\,\mathrm{F}''(\varphi)\left(\frac{d\varphi}{dt}\right)^2,$$

$$= a\frac{20}{9}\cdot\frac{a_1}{g}\frac{4\pi^2}{t_1^2}\,\mathrm{F}''(\varphi)\;(t_1 = \text{one day});$$

or if $s_1 =$ the space which a body would describe in one day by the action of a constant force $= g$,

$$\frac{d^2 r}{dt^2} = \frac{40\pi^2}{9}\cdot\frac{a_1}{s_1}\,\mathrm{F}''(\varphi)\,a;$$

or, writing r instead of its mean value a,

$$\frac{d^2 r}{dt^2} = \frac{40\pi^2}{9}\cdot\frac{a_1}{s_1}\,\mathrm{F}''(\varphi)\,r.$$

$$= -\frac{40\pi^2}{9}\frac{a_1}{s_1}\cdot\frac{3}{2}\cdot\frac{\mu}{r_1^3}\sin 2\Delta\cos\theta\sin\theta\cos\varphi\,.\,r$$

together with another term involving $\cos 2\varphi$, which will, therefore, disappear after the final integrations in determining the effect of this force on the motion of the shell, as in article 6.

The above term in the value of $\frac{d^2 r}{dt^2}$ is precisely similar to the only effective term in that part of R which arises from the disturbing force of the sun (Art. 6.), with which term it may therefore be combined. The numerical value, however, of the factor $\frac{40\pi^2}{9}\cdot\frac{a_1}{s_1}\cdot\frac{1}{2}$ is so small that the term in $\frac{d^2 r}{dt^2}$ is about $\frac{1}{250}$th of the term in R.

An exactly similar investigation is manifestly applicable to the case of the moon's action, from which it follows that the term in $\frac{d^2 r}{dt^2}$ depending on this cause will be about $\frac{1}{250}$th of the corresponding term in R.

The correction to be applied to the value of R obtained in article 6 is additive $\left(\text{since } \frac{d^2 r}{dt^2} \text{ is negative}\right)$, and, instead of the results given in that article, we shall have

$$(\mathrm{A}_3) = h\,\mathrm{A}_3\left(1 + \frac{1}{250}\right),$$

$$(\mathrm{A}_4) = h\,\mathrm{A}_4\left(1 + \frac{1}{250}\right) \text{ nearly,}$$

with similar modifications of the quantities B_3 B_4, &c.

8. If the interior fluid were homogeneous, it is manifest that the attraction of its different particles on those of the shell could have no tendency to turn the shell about the axis of y, whatever might be the position of the spheroidal axis with respect to the axis of rotation of the fluid. This will not be accurately true when the fluid is heterogeneous; but it may easily be shown that this effect may be neglected in our results. For, in the first place, the attraction between the whole shell and any fluid particle would vanish if the surfaces of equal density were all spheroidal with the same ellipticity, and can therefore only be of an order of quantities depending on the

difference of ellipticities of these surfaces. It must therefore involve a factor of the order ϵ. Secondly, the direction of the attraction of the whole shell on the fluid particle (or of the fluid particle on the shell) will pass at a distance from the axis of y not exceeding a quantity of the order ϵa. Consequently, the moment of this attraction will involve the factor ϵ^2; and such being the case for every fluid particle, the moment of the whole attraction of the fluid on the shell will contain the factor ϵ^2. Thirdly, this moment must vanish with β, and must therefore contain β as a factor. It will consequently be not greater than quantities of the order $\epsilon^2\beta$, and may be neglected.

9. From the preceding results we obtain

$$(A) = (A_1) + (A_2) + (A_3) + (A_4) \qquad \text{(First Series, Art. 6.)},$$
$$= (1+s)(A_1 + A_2) + h(A_3 + A_4)\left(1 + \tfrac{1}{250}\right).$$

When the solid shell is very thin, A_3 and A_4 are respectively much larger than A_1 and A_2, since $A_3 = \frac{A_1}{q^5 - 1}$, and $A_4 = \frac{A_2}{q^5 - 1}$. (First Series, Arts. 21, 22.). In that case, therefore, the precession will depend almost entirely on A_3 and A_4, and the introduction of the factor $1 + \frac{1}{250}$ instead of unity will give a correction amounting to nearly $\frac{1}{250}$th of the whole precession, or something less than $\frac{1}{4}$th of a second. For any but the most inconsiderable thickness of the solid shell, the correction will be much less. It may, therefore, be altogether neglected, as a quantity of the same order as the terms involving $\epsilon^2\beta$. We shall then have

$$(A) = (1+s)(A_1 + A_2) + h(A_3 + A_4),$$
$$= (1+s)(A_1 + A_2) + \frac{h}{q^5 - 1}(A_1 + A_2) \qquad \text{(First Series, Arts. 21, 22.)},$$
$$= \left(1 + s + \frac{h}{q^5 - 1}\right)\frac{q^5 - 1}{q^5} A \qquad \text{(First Series, Arts. 6, 21, 22.)}.$$

Also

$$(B) = (B_1) + (B_3),$$
$$= (1+s)B_1 + hB_3,$$
$$= \left(1 + s + \frac{h}{q^5 - 1}\right)B_1,$$
$$= \left(1 + s + \frac{h}{q^5 - 1}\right)\frac{q^5 - 1}{q^5} B.$$

Similarly

$$(B') = (B_2) + (B_4),$$
$$= \left(1 + s + \frac{h}{q^5 - 1}\right)\frac{q^5 - 1}{q^5} B';$$
$$(D) = \left(1 + s + \frac{h}{q^5 - 1}\right)\frac{q^5 - 1}{q^5} D;$$
$$(D') = \left(1 + s + \frac{h}{q^5 - 1}\right)\frac{q^5 - 1}{q^5} \cdot D'.$$

10. The coefficient of the term which gives the precession (First Series, Art. 26.) now becomes for the heterogeneous spheroid

$$\frac{(\gamma_2)}{(\gamma)}(\mathrm{A}) = \frac{\gamma_2}{(\gamma_1)+\gamma_2}(\mathrm{A}) \qquad (\text{since } (\gamma_2) = \gamma^2, \text{ (Art. 1.)},$$

$$= \frac{1}{1+k\frac{\gamma_1}{\gamma_2}}(\mathrm{A}),$$

$$= \frac{1}{1+\frac{k}{q^5-1}}\left(1+\frac{k}{q^5-1}+s\right)\frac{q^5-1}{q^5}\mathrm{A},$$

$$\left(\text{since } \frac{\gamma_1}{\gamma_2} = \frac{1}{q^5-1}\right) \quad \text{(First Series, Art. 25.)},$$

$$= \left\{1+\frac{s}{1+\frac{k}{q^5-1}}\right\}\frac{q^5-1}{q^5}\mathrm{A}.$$

If P denote the precession for the homogeneous shell considered in the preceding memoir, or (which has been shown to be the same thing) for the homogeneous spheroid of which the ellipticity $= \varepsilon$,

$$\mathrm{P} = \frac{q^5-1}{q^5}\mathrm{A} \qquad \text{(First Series, Art. 26.)};$$

and therefore if P′ denote the precession for the heterogeneous shell,

$$\mathrm{P}' = \left\{1+\frac{s}{1+\frac{k}{q^5-1}}\right\}\mathrm{P};$$

and if P_1 denote the precession which would exist if the earth were homogeneous, i. e. that of a homogeneous spheroid whose ellipticity $= \varepsilon_1$,

$$\mathrm{P} = \mathrm{P}_1\frac{\varepsilon}{\varepsilon_1};$$

and therefore

$$\mathrm{P}' = \left\{1+\frac{\eta\cdot\frac{\varepsilon_1}{\varepsilon}-1}{1+\frac{k}{q^5-1}}\right\}\frac{\varepsilon}{\varepsilon_1}\mathrm{P}_1, \quad \left(\text{since } s = \eta\left(\frac{\varepsilon_1}{\varepsilon}-1\right)\right);$$

and

$$\mathrm{P}_1 - \mathrm{P}' = \left\{\overline{1-\frac{\varepsilon}{\varepsilon_1}} - \frac{\eta\left(1-\frac{\varepsilon}{\varepsilon_1}\right)}{1+\frac{k}{q^5-1}}\right\}\mathrm{P}_1,$$

$$= \left(1-\frac{\varepsilon}{\varepsilon_1}\right)\left\{1-\frac{\eta}{1+\frac{k}{q^5-1}}\right\}\mathrm{P}_1.$$

Since η cannot be greater than unity except when the earth's crust is very thin (Art. 2.), or $q = 1$ nearly, this expression for $\mathrm{P}_1 - \mathrm{P}'$ is essentially positive, and therefore P′ is always less than P_1.

The coefficients in the terms which give the nutation are altered by the hypothesis of the earth's heterogeneity in the same ratio as the coefficient of precession.

If we take the ellipticity of the earth $= \frac{1}{300}$, the calculated value of P_1 is nearly $57''$, while the observed value of P' is only $50''{\cdot}1$. Consequently the value of the multiplier of P_1 in the above expression for $P_1 - P'$ ought to equal nearly $\frac{1}{8}$. If, then, the value of ϵ, the ellipticity of the inner surface of the solid shell, be so much less than ϵ_1 as to satisfy this condition, precession and nutation may be as well accounted for on the hypothesis of the earth's internal fluidity, as on that of its entire solidity. The required difference, however, between ϵ and ϵ_1 is such as to indicate that the least thickness of the earth's crust, compatible with the observed amount of precession, is considerable.

The discussion of the evidence which the results of the preceding investigations afford on this point, combined with other considerations, must be reserved as the subject of a future communication to the Society.

Cambridge,
Feb. 13, 1839.

XI. *Second Letter on the Electrolysis of Secondary Compounds. Addressed to* Michael Faraday, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, &c. &c. &c. By* J. Frederic Daniell, *Esq. F.R.S. For. Sec. R.S., Prof. Chem. in King's College, London.*

Received April 30,—Read May 21, 1840.

My dear Faraday,

YOU will not, I think, be surprised or displeased at my addressing you again upon the *Electrolysis of Secondary Compounds*. The whole of my very limited leisure, since my last Letter which the Royal Society did me the honour to publish in the Philosophical Transactions for 1839, has been occupied with experiments upon the subject; and I have obtained some results which I trust will not be found unworthy of the continued attention of yourself and the Society. The mode of investigation which I have adopted seems to me calculated not only to throw light upon the nature of electrolytes, but upon the mode in which the chemical elements group themselves together to constitute *Radicles* or *Proximate Principles*, the question which now seems universally to occupy the attention of chemists. I feel more than ever satisfied that the laws of electrolysis will be found to lead to the solution of this great problem.

Upon reflecting upon the constitution of the oxyacid salts, as developed in my last Letter, I conceived that it might be possible to obtain further evidence that the simple *cathion* travelled as a metal to the platinode, while the compound *anion* was passing in the opposite direction; and that means might be devised of stopping it, as it were, *in transitu*. Your beautiful experiment*, which I have often repeated with success, of precipitating the magnesia from a solution of Epsom salt against a surface of pure water, in the course of a voltaic current, suggested the mode of proceeding. According to my view of that experiment, the first electrolyte was resolved into the compound anion, sulphuric acid + oxygen, which passed to the zincode; and the simple cathion, magnesium, which on its passage to the platinode was stopped at the surface of water, from not finding any *ion*, by temporarily combining with which it could be further transferred according to the laws of electrolysis. At this point, therefore, it gave up its charge to the hydrogen of the water, which passed in the usual manner to the platinode; and the circuit was completed by the decomposition of this second electrolyte. The corresponding oxygen, of course, met the magnesium at the point where it was arrested in its progress, and, combining with it, magnesia was precipitated.

* Experimental Researches, § 494.

Now it seemed to me that this combination of the oxygen and metal was a secondary result, due to the local affinity of the two elements thus brought into juxtaposition; and was in no way connected with the primary phenomena of the current, which would have completed its course whether this combination had taken place or not, i. e. whether magnesium and oxygen had been separately evolved, or whether magnesia had been formed by the combination of the two. It also seemed probable that, although in the very slow action of this experiment this combination invariably took place, by varying the experiment so as to evolve metals possessing different degrees of affinity for oxygen, and particularly by shortening the time in which the evolution might take place, instances might be found of some portion of the metal escaping this combination, which would thus afford the most incontrovertible proof of the point to be established.

I was happy enough to succeed perfectly in my first experiment: nothing could be more simple than the arrangement.

Experiment 18.—A small glass bell, with an aperture at the top, had its mouth closed by tying a piece of thin membrane over it. It was half-filled with a dilute solution of caustic potassa, and suspended in a glass vessel containing a strong neutral solution of sulphate of copper, below the surface of which it just dipped. A platinum electrode, connected with the last zinc rod of the large constant battery of twenty cells, was placed in the solution of potassa; and another, connected with the copper of the first cell, was placed in the sulphate of copper immediately under the diaphragm which separated the two solutions. The circuit conducted very readily, and the action was very energetic. Hydrogen was given off at the platinode, or the electrode which dipped into the potassa, and oxygen at the zincode in the sulphate of copper. A small quantity of gas was also seen to rise from the surface of the diaphragm. In about ten minutes the lower surface of the membrane was found beautifully coated with metallic copper, interspersed with oxide of copper of a black colour, and hydrated oxide of copper of a light blue.

The explanation of these phenomena is obvious. In the experimental cell we have two electrolytes separated by a membrane, through both of which the current must pass to complete its circuit. The sulphate of copper is resolved into its compound anion, sulphuric acid + oxygen, and its simple cathion copper: the oxygen of the former escapes at the zincode, but the copper on its passage to the platinode is stopped at the surface of the second electrolyte, which for the present we may regard as water improved in its conducting power by potassa. The metal here finds nothing by combining with which it can complete its course, but, being forced to stop, yields up its charge to the hydrogen of the second electrolyte, which passes on to the platinode, and is evolved. The corresponding oxygen stops also at the diaphragm, giving up its charge to the anion of the sulphate of copper. The copper and the oxygen, thus meeting at the intermediate point, partly enter into combination, and form the black oxide; but from the rapidity of the action, there is not time for the whole to

combine, and a portion of the copper remains in the metallic state, and a portion of gaseous oxygen escapes. The precipitation of blue hydrated oxide doubtless arose from the mixing of a small portion of the two solutions.

Experiment 19.—The experiment was repeated with a smaller electrolytic force, six of the cells of the small constant battery being substituted for the twenty cells of the large. The action was of course much less energetic, and the membrane was found thickly coated with black oxide of copper; amongst which, however, spangles of metallic copper were plainly visible. In this case there was time for the local affinity to act, and the combination of the copper and oxygen was nearly complete.

Independently of the important conclusion which these experiments confirm, namely, that in the electrolysis of a saline electrolyte, the metal travels *as a metal* towards the platinode, while all the other constituents of the salt pass, as a compound anion, to the zincode, they present the secondary action of the local affinity in striking contrast with the primary action of the current affinity in the voltaic battery: all the decompositions and recompositions of the latter, however varied and multiplied, are limited and equalized throughout the circuit, however long, by the strictest laws; while the combinations of the latter vary with every variation of time, quantity, and energy.

The experiment with sulphate of copper was repeated and varied in several ways; as by placing the solutions in a cell separated by a perpendicular, instead of a horizontal diaphragm, &c., the particulars of which I need not dwell upon; the results being always conformable to those which I have just detailed.

I next submitted other saline solutions to the same electrolytic process, the second electrolyte being in every case solution of potassa.

Experiment 20.—From nitrate of silver, metallic silver was deposited in abundance upon the membrane, mixed with oxide of silver; gas was also visibly disengaged from the diaphragm. The whole of the oxygen was not evolved at the zincode, a portion appearing to combine, by secondary action, with the oxide of silver in the nitrate, and forming a *peroxide*. It was precipitated as a brown powder, which dissolved in nitric acid with evolution of oxygen.

The experiment was varied by placing the solution of nitrate of silver in a small porous earthenware cell, which, upon examination afterwards, was found studded with minute filaments of silver, and coloured by the oxide; but this apparatus was not found to be so well adapted to the purpose as the membrane, which, from its thinness, exhibited the results of the necessarily superficial action to perfection.

Experiment 21.—Nitrate of lead afforded similar results. Metallic lead was deposited upon the bladder with oxide of lead, and, as with the silver, the oxygen was absorbed at the zincode with the precipitation of peroxide of lead.

Experiment 22.—Protosulphate of iron threw down a copious black precipitate upon the diaphragm, which, when examined with a lens, exhibited bright metallic points; but oxidation took place so rapidly, after the membrane was withdrawn from the cell, that the metallic lustre quickly disappeared.

Experiment 23.—From sulphate of palladium I obtained in a similar way metallic palladium; but it had little lustre, and did not exhibit the beautiful appearance of the copper and silver.

Experiment 24.—Protonitrate of mercury, of all the saline electrolytes which I submitted to experiment, exhibited the most striking phenomenon. Not only were metallic globules formed in abundance upon the membrane, but when the exterior cell was of glass a shower of small globules was seen to fall from the diaphragm during the whole continuance of the experiment.

Experiment 25.—I now tried sulphate of magnesia, not without hopes of finding metallic magnesium amongst the products of the experiment; but abundance of magnesia alone was precipitated upon the diaphragm. I hence conclude that the affinity of the metal for oxygen is too strong in this instance to allow of any portion escaping combination.

Combining the proofs which I have now adduced with the totally different class of evidence which I brought forward in my last communication, I think it must now be admitted that I have established the important point that, considered as electrolytes, the inorganic oxyacid salts must be regarded as compounds of metals, or of that extraordinary compound of nitrogen and four equivalents of hydrogen to which Berzelius has given the name of ammonium, and compound anions, which are exactly analogous to the simple anions, chlorine, iodine, &c. of the haloide salts; and that this evidence goes far to establish experimentally the hypothesis, originally brought forward by Davy, of the general analogy in the constitution of all salts, whether derived from oxyacids or hydroacids.

Before I proceed with the details of my experimental researches, I must beg your indulgence for a few remarks upon the subject of nomenclature, a change in which seems absolutely necessary from such a change of views; and without which it seems scarcely possible to avoid circumlocutions of the most embarassing kind, of which you will find abundant evidence in what I have already written. In speaking of salts as electrolytes, we greatly need significant expressions for the compound anions with which their metallic cathions are combined. In referring to sulphate of copper as an electrolyte, I have been obliged to designate its anion as sulphuric acid + oxygen, than which nothing can be more clumsy and, as far as the significancy of the words is concerned, erroneous. After much reflection upon the subject, it has occurred to me that the term *ion* which you have introduced to designate indifferently the two constituents of an electrolyte, and which you have further compounded into *anion* and *cathion*, to designate the elements which travel respectively to the *anode* and *cathode* of the voltaic battery, might be adopted as a general termination to denote the compounds which in the electrolysis of a salt pass to the zincode, and that they might be specifically distinguished by prefixing the name of the acid slightly modified: thus, electrolytically considered, the sulphate of copper might be called the *oxysulphion* of copper; nitrate of potassa, the *oxynitrion* of potassium. The *oxysulphion* of the former, which would travel to the zincode of the battery, would con-

sist of $\overline{S + 4\,O}$; the *oxynitrion* of the latter, of $\overline{N + 6\,O}$. The *oxysulphion* of *ammonium*, or $(S + 4\,O) + (N + 4\,H)$, would designate the sulphate of ammonia with one equivalent of water.

The composition of words upon this principle is so extremely obvious, that I think I need not illustrate it further. I do not, of course, propose that the common designation of salts should be in all cases altered, but only when it may be necessary to speak of them in their electrical relations; and the termination *ion* will have the convenience of immediately suggesting these relations.

No one, I think, can be more alive than myself to the inconvenience of unnecessary alterations of nomenclature; they constitute an evil of no slight consideration, and have lately been, in my humble judgement, too much indulged in by chemical philosophers; but when it is sought to introduce new views and generalizations upon sufficient experimental evidence, they become absolutely necessary, and operate beneficially in assisting the mind to break through the trammels of a train of thought which is invariably attached to old expressions. Suffer me to make a moderate use of this new nomenclature in what I have further to communicate.

One of the principal objections to Davy's theory of the salts has always been the impossibility of isolating, and preserving in a free state, the compounds of acids with the additional equivalent of oxygen which it supposed; and in their electrolysis we find that the ions invariably evolve their oxygen at the zincode. It occurred to me that evidence of their isolation might be found in the beautiful experiments of M. Thenard*, which led to the discovery of oxygenated water. It will be remembered, in fact, that the first conclusion which that eminent philosopher drew from them was that the phenomena were due to such combinations of the acids with oxygen; and it was only after a long course of further investigation that he rested in the opinion that the extra equivalent of oxygen was in all cases united to the water with which they were mixed. He at the same time allowed that the combination was greatly more permanent when any acid was present. I was in hopes that I might have obtained further evidence of the fact from an apparatus which I contrived for the purpose, but the results were not conclusive, and I shall not trouble you with the details.

Experiment 26.—The apparatus consisted of a double diaphragm cell, one of the branches of which was placed in a vessel capable of holding a mixture of salt and pounded ice, by which it might be cooled to 0° Fahr. The cell was charged successively with dilute sulphuric acid, dilute phosphoric acid, and solution of sulphate of soda. The zincode was cooled by the freezing mixture, and in no instance was the oxygen evolved in proportion to the hydrogen at the platinode; and in some cases the evolution appeared to be entirely arrested. The conducting power of the circuit, however, was very much lowered by the reduction of the temperature, and was finally destroyed by the formation of ice, so that no very striking result could be obtained.

* Annales de Chimie et de Physique, tomes viii. ix.

You will doubtless remember that in my last letter I communicated to you the anomalous and perplexing results which I obtained from the electrolysis of different mixtures of sulphuric acid and water. Every analogy led me to expect that the hydrated acid would prove an *oxysulphion of hydrogen*, and that hydrogen would be evolved at the platinode, and an equivalent of sulphuric acid with an equivalent of oxygen at the zincode. But after a great number of experiments, the results of which were consistent within certain limits, I was obliged to conclude that, for each equivalent of oxygen and hydrogen evolved, only one fourth equivalent of sulphuric acid passed to the zincode. Now it would be an extravagant hypothesis to suppose that another compound anion was evolved upon this occasion, consisting of one equivalent of sulphur and seven equivalents of oxygen, as the result of the decomposition of an electrolyte consisting of one equivalent of sulphuric acid united to four equivalents of water. The subject obviously required further investigation; and as the question appeared to me to be of first-rate importance, I bestowed much time and labour in repeating and modifying the experiments*. It would uselessly occupy your time to enter upon the details of this course of investigation; but I will briefly describe my mode of proceeding, some of the difficulties which I met with, and the means by which I attempted to obviate them.

Experiment 27.—The apparatus which I employed was of the same general nature as that which I have previously described. The gases evolved were measured and estimated in the usual manner; and the changes which took place in the relative proportions of acid and water in the two cells were approximately determined by weight, by neutralization with carbonate of soda, or by precipitation with baryta. The connecting tube (Experiment 8.) was improved by a small perpendicular tube inserted at the top of the bend, which allowed room for the expansion of the included liquid from the heat evolved, which was at times not much short of the boiling point of water. Without this precaution there was danger of some of the liquid being mechanically forced through the diaphragm, and even of rupture of the membranes.

The determinations by weight were rendered uncertain from the evaporation which took place from the surface of the liquid in the two cells at the high temperature; and it was not easy to avoid loss from the fine spray which was forcibly thrown up by the escape of the gases at the electrodes. Upon the whole, however, there was no reason to conclude that water passed to the platinode while the sulphuric acid accumulated at the zincode.

The determinations by neutralization and precipitation were much more satisfactory, and generally consistent with those which I had previously obtained. Upon a review of the whole I am quite certain that in the electrolysis of dilute sulphuric acid the quantity of acid which passes to the zincode with one equivalent of oxygen,

* In all these experiments I was greatly indebted to the able assistance of my friend and pupil Mr. W. A. Miller, without which it would have been impossible for me to have gone through the almost numberless operations.

and for which one equivalent of hydrogen is given off at the platinode, is not more than a fourth of an equivalent, nor less than a fifth. The majority of the results, however, indicate the former proportion.

Could it be possible that a whole equivalent of acid was, in fact, carried over to the zincode, but was counteracted by that species of endosmose which I have before described, by which three fourths of an equivalent might be transported in the opposite direction to the platinode?

As this transfer is, however, more of a mechanical than of an electrolytical nature, it could not well take place without a visible rise of the liquid in the platinode cell, and none such was ever observed. It occurred to me that this question might be set at rest by multiplying the diaphragms between the electrodes. I had already succeeded, as I believed, in stopping this action by two diaphragms; it was impossible but that by adding two more some change must be produced in the amount, at least, of the endosmose, if any such really took place.

Experiment 28.—I connected the two electrode cells with a third cell by means of a second bent tube with two diaphragms, and all were charged with dilute sulphuric acid of the specific gravity 1076. The experiment was continued till seventy-one cubic inches of mixed gases had been collected in the voltameter, and lasted five hours. The acid in the three cells and two connecting tubes were then separately and carefully neutralized. The acid in the two latter and centre cell had not undergone the slightest change in quantity; but in the platinode cell it was deficient, and in the zincode cell in excess exactly eight grains.

Experiment 29.—The experiment was immediately repeated with the same acid, and two cells with one connecting tube. The same quantity of gases was collected in two hours and forty minutes, and the transfer of acid from the platinode to the zincode was found to be exactly the same without any change in the connecting tube. No doubt, therefore, can now exist that the transfer of the acid is electrolytic in its nature.

Experiment 30.—I next repeated the experiment with an amalgamated zinc, a copper, and a platinum zincode of the same dimensions. The quantity of hydrogen gas collected in each case was 23·7 cubic inches (or half an equivalent). The times were respectively forty-six minutes, fifty-four minutes, and fifty-five minutes; and the transfer of acid from the platinode was found to be in each about five grains, or one fourth equivalent for an equivalent of the gas. The zinc and copper electrodes had lost weight in proportion to the equivalent of the gas.

These last results are not without interest as regards the electromotive force of the battery. The absorption of the oxygen by the copper decreases but little the obstructions of the current, by the removal of an opposing element; but not only is the absorption of the same element effected by the zinc electrode, but an energy of one fifth, as measured by the time, added to the current.

Experiment 31.—The difference of this mere absorption of oxygen by a metal, and

the additional energy of zinc in a voltaic circuit, is beautifully exemplified in the following experiment. I took ten cells of a small constant battery and charged them in the usual way, only substituting three amalgamated rods of grain tin, which had been previously weighed, for three of the zinc rods. The circuit was completed with a voltameter, and twenty-five cubic inches of mixed gases were collected in one hour; and the three rods were found to have lost respectively twenty-two grains, twenty-two grains, and twenty-one grains, or about an equivalent each for the gas. The circuit was immediately closed with the voltameter and the seven remaining zinc rods, and the same quantity of gas was collected in eight minutes. Now with regard to the mere oxidation of the metal and the removal of the oxide by the acid, everything proceeded apparently the same with the tin rods as with the zinc, and yet we find that instead of adding anything to the energy of the current, they reduced its efficiency to less than one seventh.

I cannot help remarking, by the way, that this is a result which the advocates of the *contact theory* would find it very hard to reconcile to their principles. According to their views the electromotive force of tin and copper is very little, if anything, inferior to that of zinc and copper; the resistances of all parts of the circuit are the same, and yet the addition of a few more couples of tin and copper instead of adding anything to the intensity of the current would stop it altogether.

But to return to the more immediate object of this paper. We have seen that whatever be the metal of the zincode, the quantity of acid which travels to it in the electrolysis of dilute sulphuric acid is sensibly the same, and we have yet found nothing to explain the extraordinary proportion which it bears to the gases evolved, or to make it harmonize with the results of other electrolytes.

Experiment 32.—Dilute phosphoric acid was next submitted to similar experiments. Some pure acid was prepared for the purpose; but I was at a loss for some time for a convenient and accurate method of determining the quantity of acid in any solution. This I at last satisfactorily effected by mixing it with pure oxide of lead, careful evaporation and ignition, according to the method of Professor Rose.

In the first experiment, the solution consisted of one part by weight of the glacial acid to sixteen parts of water. It conducted badly, and only 17·75 cubic inches of mixed gases (or one fourth of an equivalent) were collected in three hours and twenty-five minutes. The result was accompanied by a circumstance which I must make the subject of a future communication.

Experiment 33.—The experiment was repeated with a solution which consisted of one part glacial acid to eight of water. Twenty-four cubic inches of the gases were now collected in $2\frac{3}{4}$ hours, and the quantity of acid which had travelled from the platinode to the zincode was found to be 3·6 grs., which again is in about the proportion of one quarter of an equivalent to one equivalent of the gases. So that with phosphoric acid we have the same anomalous proportions as with the sulphuric acid.

Experiment 34.—In a postscript to my last letter I informed you that I had ascer-

tained that, in aqueous solutions of the fixed alkalies, the oxides travel in the contrary direction to that of the acid in the dilute sulphuric acid, and that the amount thus transferred to the platinode was certainly below that of the equivalent of the gases evolved. I lost no time in endeavouring to ascertain the exact proportions; but met with some difficulty from the rapid action of the alkaline solutions upon the membranes of the connecting tube. This was at last overcome by closing the apertures with ground disks of porous earthenware tied down with pieces of calico. The mean of two experiments, in each of which 35·4 cubic inches of the mixed gases were collected, showed, by saturation with sulphuric acid, five grains of potassa transferred to the platinode, or something less than one fifth of an equivalent for an equivalent of the gases.

Experiment 35.—A solution of baryta of the specific gravity 1008·5 was found to conduct very badly, and only 7·3 cubic inches of mixed gases could be collected in five hours. The transfer of baryta to the platinode was ascertained by precipitation with sulphuric acid to be 1·53 grs., whence we may calculate that the transfer for one equivalent of the gases would be 14·84, or about one fifth equivalent: for

Mixed gases.		Mixed gases.		Baryta.		Baryta.
7·3	:	70·8	: :	1·53	:	$14{\cdot}84 = \frac{76}{5}$ nearly.

Experiment 36.—A solution of strontia of the specific gravity 1007 proved to be a worse conductor even than the preceding: but the experiment was allowed to proceed for twenty-one hours, when 32·7 cubic inches of mixed gases were found to have been collected. The platinum platinode was covered with crystals of hydrate of strontia. The liquid in the zincode cell was scarcely rendered turbid by sulphuric acid, while the solution in the platinode cell afforded 14 grs. of sulphate of strontia, so that the whole of the strontia, or 7·9 grs., had passed over to the platinode. Scarcely a globule of gas was evolved from the electrodes at the time when the experiment was stopped, although the battery was still capable of evolving gas in the voltameter at the rate of about one cubic inch per minute.

Now, if we suppose that no gas was evolved after the whole of the strontia had passed over, the transfer was in the proportion of about one third equivalent for an equivalent of the gases, for

Mixed gases.		Mixed gases.		Strontia.		Strontia.
32·7	:	70·8	: :	7·9	:	$17{\cdot}1 = \frac{52}{3}$ nearly,

but as it is probable that there was some excess of gas from the long continuance of the experiment, the result cannot be held to differ materially from the preceding.

Experiment 37.—I now took a solution of baryta of the specific gravity 1033 and ascertained, by precipitation with sulphuric acid, that a measure of it contained 26·9 grs. of baryta. I charged the cell with an equal measure of the same solution, and placed it in the voltaic circuit with a voltameter, with the intention of carrying on the experiment till the whole of the baryta had been transferred, as was the case with the strontia of the last experiment. The results were not examined till the expiration of forty-eight hours. The quantity of gases collected amounted to 139 cubic

inches, and abundant crystals of hydrate of baryta were found around the platinode. The evolution of gas in the cell had ceased, but the battery was afterwards found to be still capable of generating gas in the voltameter at the rate of $2\frac{1}{2}$ cubic inches in ten minutes. Upon attempting to remove the cell the membranes had been so much acted upon and softened that they burst; so that it was impossible to ascertain whether the zincode cell still contained any baryta. But as it is most probable, from the duration of the experiment, that the transfer was complete, as in the case of the strontia, we have still the data for a calculation, from which it would appear that about one fifth of an equivalent of baryta was transferred for an equivalent of the mixed gases: for

Mixed gases.		Mixed gases.		Baryta.		Baryta.
139	:	70·8	: :	26·9	:	13·7

which is nearly equal to $\frac{75}{5}$ or 15.

Without ascribing to these results an accuracy which, with all the pains I could bestow, the mode of experimenting did not admit of, we may conclude, quite within the limits of possible experimental error, that in the electrolysis of aqueous solutions of acids or the fixed alkalies, the evolution of an equivalent of oxygen at the zincode is accompanied in the first instance by an accumulation of acid, which cannot exceed one fourth of an equivalent, or fall short of one fifth; and the evolution of hydrogen at the platinode in the second case, by an accumulation of alkali to an amount within the same limits.

Now with regard to the alkaline experiments it is, I conceive, still more impossible to believe that the cathion which travelled to the platinode was an association of an equivalent of potassium, or other metal, with three equivalents of hydrogen (the fourth being due to the reaction of the metal upon the water), than to believe that, in the sulphuric acid experiments, the anion which went to the zincode was a compound of one equivalent of sulphur and seven equivalents of oxygen. What must we then conclude?

Shall we suppose that the electrodes of the battery are endued with an attractive power for different kinds of matter, in virtue of which free acids pass to the zincode and free alkalies to the platinode? Shall we further imagine that the proper electrolytic force of the current, with regard to water, is assisted by this adventitious force, so that water which contains either free acid or alkali is a much more efficient conductor, and is more abundantly decomposed than pure water?

I will not dwell upon other objections to this hypothesis, but I must own my utter unwillingness to return to a notion which I think your "Experimental Researches" have so completely subverted; and it would require much stronger considerations than the pressure of the present difficulty to make me abandon your definition of the electric current as "an axis of power having contrary forces, exactly equal in amount, in contrary directions*." I am, moreover, not without hopes that you will consider

* Experimental Researches, § 517.

the results of my next experiment as furnishing a key to the mystery which has occasioned me so much perplexity and labour.

Experiment 38.—I was naturally anxious to complete my electrical examination of the sulphates by the electrolysis of the *bi-salts* of this class, and for this purpose I made a strong solution of pure crystallized bi-sulphate of potassa; and carefully ascertained its neutralizing power, by the alkalimeter, and also, by evaporation and ignition with carbonate of ammonia, the quantity of *neutral* sulphate which a certain measure of it contained. I then exposed an equal measure of the solution to the voltaic current in each of the arms of the double diaphragm cell till 70·8 cubic inches of the mixed gas had been collected. Half of the solutions from the zincode and platinode were then separately neutralized, and half evaporated and ignited.

Upon a calculation for the whole quantity, the zincode solution appeared to have gained eighteen grains of acid, and the platinode to have lost nineteen grains, while the zincode had lost fifteen grains of neutral sulphate, which are equivalent to 8·2 grs. of potassa, and the platinode had gained something less.

Experiment 39.—The experiment, the results of which appear to me to be extremely important, was immediately repeated, and the losses and gains of the zincode and platinode solutions corresponded still more closely than before. With regard to the acid, the gain of the zincode was eighteen grains, the loss of the platinode nineteen grains. With regard to the potassa, the gain of the platinode was 9·9 grs., and the loss of the zincode exactly equal.

Hence it appears that the law of the electrolysis of the *bi-salt* is very different from that of the *neutral* salt: the solution conducted very well, but as estimated by the potassa transferred to the platinode, not more than one fifth of an equivalent was decomposed for an equivalent of hydrogen evolved at the same electrode. This was accompanied by the transfer of half an equivalent of acid to the zincode, where a whole equivalent of oxygen was also given off.

I think that we cannot hesitate to admit that, in this case, the current divided itself between two electrolytes, and that a part was conducted by the neutral sulphate of potassa, and a larger part by the sulphuric acid and water, after the manner of the solutions upon which we have dwelt so long. It is a well known fact that the voltaic current will divide itself between two or more metallic conductors in inverse proportion to the resistance which each may offer to its course; and that it does not in such cases choose alone the path of least resistance; and, in a former communication* to you, I pointed out some curious instances of *diverted currents* in which liquid electrolytes were concerned. I am not aware that such a division of a current between two electrolytes in the same solution has ever before been pointed out, but analogy would lead one to expect it.

With this new view let us now revert first to the experiments with the dilute sulphuric acid. If we regard this acid as a mixture of the two electrolytes, *oxisulphion*

* Philosophical Transactions, for 1837, p. 119.

of hydrogen H + (S + 4 O) and water (H + O), we may derive an explanation of the phenomena much more consistent with the laws of electrolysis which you have established than any which has yet presented itself. In this case the current divides itself between the two so as to effect the decomposition of three equivalents of water and one equivalent of the *oxisulphion*, and thus four equivalents of hydrogen will be evolved at the platinode, and four equivalents of oxygen with one equivalent of sulphuric acid at the zincode.

The explanation of the alkaline decompositions is no less easy. The solution of potassa, for instance, is a mixture of the two electrolytes, oxide of potassium (P + O) and water (H + O): the current divides itself between the two, and electrolyses one equivalent of the former, and three equivalents of the latter. Four equivalents of oxygen make their appearance at the zincode, one derived from the oxide of potassium and three from the water; and four equivalents of hydrogen at the platinode, one of which is due to the secondary action of the metal upon the water.

In the second place, let us resume the consideration of the experiment with bisulphate of potassa. We have already seen reason to conclude that the solution of this salt acts as a mere mixture of neutral sulphate and sulphuric acid in water; and that the current is conducted in a small part by the decomposition of the salt, and still more abundantly by the dilute acid. We must now consider the solution as containing three electrolytes, namely *oxisulphion of potassium* P + (S + 4 O), *oxisulphion of hydrogen* H + (S + 4 O), and water (H + O), and the results of the electrolysis agree very well with the supposition that the current divides itself between the three, so as to effect the decomposition of one equivalent of *oxisulphion of potassium*, one equivalent of *oxisulphion of hydrogen*, and two equivalents of water.

The two following Tables will exhibit the results of the actual experiment, and those of an equivalent calculation founded on the hypothesis; from a comparison of which it will appear that, considering the numerous sources of inaccuracy in the experiments, the agreement is very close: taking, indeed, the different quantities of sulphuric acid as the more certain measure of the effects it is almost perfect.

TABLE I. Experimental Results.

Neutral salt.		Acid.		Alkali.	Free acid.
Before the electrolysis the solution contained					
104·2	=	47·3	+	57	44
Zincode after Electrolysis.					
86·0	=	39·0	+	47	64
(−18·2)		(−8·3)		(−10)	(+20)
Platinode after Electrolysis.					
122·0	=	55	+	67	26
(+18·2)		(+8·3)		(+10)	(−18)

TABLE II. Calculated Results in Equivalents.

Before the electrolysis the solution contained

Neutral salt.		Acid.		Alkali.	Free acid.
96·8	=	44	+	52	44

Zincode after Electrolysis.

72·6	=	33	+	39·6	66
(−24·2)		(−11·0)		(−13·2)	(+11×2)

Platinode after Electrolysis.

121·0	=	55·0	+	66·0	22
(+24·2)		(+11·0)		(+13·2)	(−11×2)

According to this view, the electrolysis of an equivalent of bisulphate of potassa in water would yield at the zincode four equivalents of oxygen, one of which would be derived from one equivalent of *oxisulphion of hydrogen*, a second from one equivalent of *oxisulphion of potassium*, and two from two equivalents of water, together with two equivalents of sulphuric acid which would remain in solution: and at the platinode, four equivalents of hydrogen, one from the *oxisulphion of hydrogen*, a second from the secondary action of an equivalent of potassium upon the water of the solution, and two from the electrolysed water, together with one equivalent of potassa in solution.

I must again guard myself by stating, that I do not think the accuracy of the experiments sufficient to determine whether the proportions of the electrolytes decomposed are, as I have just represented them, three to one, or four to one; or whether the proportions with regard to water are always the same with respect to them all; but if so, and the results justify the suspicion, the electrolyte, water, must exercise a limiting power upon other electrolytes, which is well worthy of further careful investigation. The reaction also of other electrolytes upon water, by which its conducting power is so greatly increased, still remains involved in obscurity.

The electrolysis of sulphate of copper, under certain circumstances, is attended by a remarkable effect, which I think may be explained by the principle just laid down. You will remember that in my account of the construction of the *constant battery** I attributed the precipitation of the copper from the sulphate to "the deoxidating power of the nascent hydrogen;" but from the first I have been generally struck with the beautiful crystalline arrangement and close compact structure of the metallic deposit. This is so remarkable, that in the first small sheet of copper which I stripped from a platinum electrode, and which is now in my possession, I noticed that the scratches of the file, which the latter bore, were perfectly moulded upon it. This is unlike what one would have expected, *à priori*, from the precipitation of a metal, by a

* Philosophical Transactions, 1836, p. 117.

gas adhering to the surface of the electrode, and is much more consistent with, what we now know to be the fact, the immediate deposit of the metallic cathion upon the platinode without the intervention of the gas. I had also remarked, at times, without being able to account for it, that the texture of the copper was spongy and little coherent.

The beautiful application of this observation by Professor Jacobi and Mr. Spencer to the formation of fac-similes of medals and other *volta-types*, as I have proposed to call them, has made us better acquainted with the minutest details of this kind of precipitation; and every one who has attended much to the process, must have observed that, in conducting it in the usual way, after the same solution has been a long time in use, the deposit of compact copper became mixed with copper in a loose, pulverulent state, which can be brushed off the mould with the fingers, and greatly interfered with the beauty of the result. This arises from the accumulation of acid in the solution from the decomposed sulphate; and I have no doubt that *oxisulphion of hydrogen* being thus formed, the current is divided between it and the *oxisulphion of copper*; and that the pulverulent copper is owing to the secondary action of the hydrogen disengaged from the former upon the solution.

Experiment 40.—I have from the first prevented this injurious effect in Mr. Spencer's process, by making the precipitation in a separate cell, containing the sulphate of copper, connected with two or three cells of the constant battery by a copper zincode; by this arrangement the copper was dissolved at the zincode as fast as it was deposited at the platinode, and the accumulation of acid prevented.

I shall now only trouble you with a brief account of two or three more experiments, the results of which throw some additional light upon the electrolysis of secondary compounds.

Experiment 41.—I was anxious to ascertain in what manner the *carbonates* would conduct themselves as electrolytes, and particularly the relation which they would exhibit to the *oxalates*. For this purpose the carbonates of potassa, soda, and ammonia were subjected to the voltaic action in the double diaphragm tube, the zincode cell of which was sometimes connected with a Liebig's tube, for the purpose of absorbing the carbonic acid; and carbonic acid and oxygen were found to be given off in equivalent proportions to the alkali and hydrogen at the platinode; proving these salts to be *oxicarbions* of potassium, sodium, and ammonium respectively.

Experiment 42.—Oxalate of ammonia, treated in the same way, yielded nothing at the zincode but carbonic acid, and ammonia with hydrogen at the platinode. When the constitution of the oxalic acid is remembered, this result will be very intelligible; for

$$\overset{\text{Oxalic acid.}}{(2\,C + 3\,O)} + O = \overset{\text{Carbonic acid.}}{2\,(C + 2\,O)},$$

the one equivalent of oxygen being quite sufficient to convert the one equivalent of oxalic acid into two equivalents of carbonic acid.

Nevertheless the oxalate must be looked upon as an *oxalion of ammonium*,

$$(C + 4\,O) + (N + 4\,H)$$

and the production of the carbonic acid as a secondary effect.

I am inclined to believe that the discovery of a mode of electrolyzing any of the numerous combinations of hydrocarbon, so as to isolate that principle or carbon, would tend more to advance our knowledge of organic compounds than any other single step which could be taken; and I was not without hopes that the *sulpho-vinates* might afford some indications which might lead to such a process.

Experiment 43.—I therefore subjected some pure sulpho-vinate of potassa, for which I was indebted to the kindness of Mr. Hennel, to the action of the voltaic current in the double diaphragm cell. It proved to be a very good conductor, and the results were not ambiguous. Sulphovinic acid and oxygen were collected at the zincode, and potassa and hydrogen, all in equivalent proportions, clearly establishing this salt as a *sulphovinion of potassium*.

Permit me, now, before I close this letter, to make a few observations upon a subject which strikes me as closely connected with the compound electrolytes upon which I have dwelt at such length; I mean the origin of the currents in Becquerel's *battery*. I never could bring myself to believe that the mere combination of the acid and alkali could give rise to such a current, and have always thought that you have abundantly proved its impossibility. And yet there is now a mode of viewing such a combination, which I think may explain the phenomena.

Experiment 44.—I have carefully repeated and varied the experiments, for the purpose of possessing myself accurately of the facts. I chiefly made use of solutions of nitric acid and potassa placed on either side of a diaphragm of bladder in a divided glass cell, which could be firmly bound together with brass clamps. Platinum electrodes were placed on either side of the diaphragm.

I satisfied myself not only of the usual phenomena with the galvanometer, but that oxygen was evolved on the potassa side, and hydrogen on the nitric acid side. The former I had no difficulty in collecting in an inverted closed tube; the latter was recognised by its secondary action upon the nitric acid. Nitrate of potassa was of course formed at the junction of the acid and alkali. Now let us recollect what nitrate of potassa is in its electrical relations: it is an *oxinitrion of potassium* $(N + 6\,O) + P$. Aqueo-nitric acid is also an *oxinitrion of hydrogen* $(N + 6\,O) + H$; and potassa is oxide of potassium $(P + O)$. In their local action upon each other, the acid and the alkali are both decomposed; the oxinitrion of the former combines with the metal of the latter, and water is formed by the union of the hydrogen and oxygen. This water there is no difficulty in regarding as a separate and secondary product, in as much as the salt is incapable of combining chemically with it.

When a circuit, however, is formed with proper conductors, the compositions and recompositions take place through a series of connected particles, as in the manner of all other electrolytic conduction: and the oxygen and hydrogen instead of com-

bining together, as in the local action, are respectively evolved at the zincode and platinode. The following diagrams may perhaps assist in explaining my notion of the origin and connection of the current.

$$\overbrace{OP}\,\overbrace{OP}\,\overbrace{OP}\,\overset{A}{\underset{B}{|}}\,(N+\overbrace{6O)H}\,(N+\overbrace{6O)H}\,(N+\overbrace{6O)H}$$

Let $\overbrace{OP}$ and $(N+\overbrace{6O)H}$ represent the two electrolytes on the opposite sides of the diaphragm A B before action; after action has commenced they may be represented thus:

$$O\overbrace{PO}\overbrace{PO}\overbrace{P}\overset{A}{\underset{B}{|}}\overbrace{(N+6O)H}\overbrace{(N+6O)H}\overbrace{(N+6O)H}$$

May I not ask, in conclusion, whether such a current does not again defy any explanation upon the contact theory?

Believe me, dear FARADAY,

Very faithfully yours,

J. F. DANIELL.

King's College,
29*th April*, 1840.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY,

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

OBSERVANDA.

Height of the Cistern of the Barometer above the plinth at Waterloo Bridge....83 feet 2 inches.
_____ above the mean level of the sea .....97 feet.
Height of the receiver of the Rain Gauge above the court of Somerset House ..79 feet.
The External Thermometer is 2 feet higher than the Barometer Cistern.
The Thermometers are graduated to Fahrenheit's scale.
The Barometer is divided into inches and tenths.
The Hours of Observation are of Mean Time, the day beginning at Midnight.
The *daily* observations of the Barometer are *not* corrected.
The *monthly means* are corrected for capillarity and temperature by the Table contained in Mr. Baily's paper in *Phil. Trans.* for 1837.

METEOROLOGICAL JOURNAL FOR JULY AND AUGUST, 1839.

1839.		9 o'clock, A.M.			3 o'clock, P.M.			Dew Point at 9 A.M. dog. Vals.	Diff. of Wet and Dry Thermometer.	External Thermometers.				Rain in Inches, fallen by 9 A.M.	Direction of the Wind, at 9 A.M.	REMARKS.
		Barometer uncorrected.		Att. Ther.	Barometer uncorrected.		Att. Ther.			Fahrenhe't.		Self-registering				
		Flint Glass.	Crown Glass.		Flint Glass.	Crown Glass.				9 A.M.	3 P.M.	Lowest	Highest			
JULY	M 1	30.312	30.304	58.2	30.308	30.302	60.4	46	06.3	55.2	61.8	48.6	55.5		N	A.M. Cloudy—light brisk wind. P. M. Fine—light clouds & wind. Evening, Cloudy.
	T 2	30.308	30.300	58.3	30.298	30.290	60.7	50	04.6	56.9	63.3	51.8	63.3		W	Overcast—light wind throughout the day. Ev. Very slight rain.
	W 3	30.308	30.300	62.3	30.260	30.252	64.0	53	07.1	61.3	68.6	53.5	64.0		N	Cloudy—light breeze throughout the day. Evening, Overcast.
	T 4	30.228	30.222	72.2	30.184	30.178	66.7	55	06.1	63.9	73.3	57.0	66.7		NE	Fine—light clouds and wind throughout the day. Ev. Overcast.
	F 5	30.208	30.200	64.3	30.146	30.140	66.5	58	06.6	62.8	69.3	57.8	73.8		E	Overcast—light breeze throughout the day. Ev. Fine & clear.
	S 6	30.040	30.034	75.8	29.946	29.940	69.2	62	07.8	66.8	77.9	56.5	80.3		SW	A.M. Fine—light clouds. P.M. Fine—nearly cloudless—light wind. Evening, Fine and clear.
	⊙ 7	29.860	29.852	67.2	29.788	29.784	69.2	60	07.2	64.9	73.8	59.0	78.2		SE	A.M. Cloudy—lt. breeze. P.M. Lightly overt.—brisk wind. Ev. Dark & heavy clds.—thunder & lightning the whole evening—heavy rain.
	M 8	29.664	29.658	71.0	29.654	29.650	68.9	61	08.0	66.2	67.4	61.0	74.4	.094	SSE	A.M. Cloudy—light brisk wind, with heavy shower. P.M. Fine—light clouds and wind. Evening, Fine and clear. (rain.
	T 9	29.738	29.734	70.7	29.748	29.742	69.4	60	08.1	62.8	65.4	56.2	73.2	.044	S	A.M. Fine—lt. clds.—brisk wind. P.M. Cldy.—very h. wind. Ev. Light
○	W10	30.008	30.004	76.8	29.996	29.990	66.8	59	08.3	63.7	66.7	53.3	77.3	.022	S	Cloudy—high wind throughout the day. Evening, Fine—light clouds and wind.
	T11	30.016	30.012	80.6	29.946	29.940	69.6	61	08.5	65.4	70.7	58.6	78.3		S	A.M. Fine—light clouds—brisk wind. P.M. Fine and cloudless—brisk wind. Evening, Overcast.
	F12	29.852	29.848	69.5	29.880	29.874	69.5	61	07.2	67.4	68.5	62.4	71.6		S	Cloudy—high wind throughout the day. Ev. Overcast—light rain.
	S 13	30.116	30.110	76.2	30.136	30.130	69.9	60	08.6	64.3	72.2	56.6	73.2	.033	S	Fine—light clouds & breeze throughout the day. Ev. Fine & clear.
	⊙ 14	30.026	30.020	74.9	29.914	29.908	69.2	54	10.5	67.7	68.7	58.2	73.6		E	A.M. Cloudy—light wind. P.M. Overcast—brisk wind. Ev. Dark clouds—high wind. (Cloudy—light rain.
	M15	30.016	30.010	73.4	30.070	30.062	70.2	61	08.3	65.2	70.7	57.9	70.4	.069	SW var.	Fine—light clouds, with light brisk wind throughout the day. Ev.
	T 16	30.198	30.192	69.8	30.192	30.186	68.7	56	07.8	64.2	69.8	56.3	69.3		W	A.M. Cloudy—light rain and wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
	W17	30.100	30.096	77.3	29.934	29.926	69.2	58	09.4	65.4	67.8	53.2	68.7		E var.	A.M. Fine—lt. clouds and breeze. P.M. Cloudy—brisk wind. Ev. Overcast—much lightning, with heavy rain at 4 p. m. (& clear.
	T 18	29.550	29.542	67.3	29.574	29.568	69.4	64	05.2	67.0	66.8	62.3	72.0	.222	SE	A.M. Ovct.—heavy rain—h. wind. P.M. Cldy.—very h. wind. Ev. Fine
	F 19	29.636	29.630	66.9	29.666	29.658	67.6	59	03.3	63.4	67.4	58.2	68.3		SW var.	A.M. Cloudy—high wind—also throughout the night. P.M. Fine—lt. clouds—high wind. Ev. Overcast—slight rain—high wind.
	S 20	29.754	29.748	67.2	29.788	29.784	68.2	59	06.7	63.3	66.7	60.0	67.7		S	A.M. Overcast—brisk wind. P.M. Dark heavy clouds—light rain —high wind. Evening, Cloudy.
	⊙ 21	29.912	29.906	66.9	29.942	29.938	68.5	58	07.2	63.5	68.7	56.6	72.0		S	Cloudy—light wind throughout the day. Ev. Fine—light clouds.
	M22	30.100	30.092	68.6	30.096	30.088	67.2	59	07.1	64.4	68.3	56.9	72.8		SSE	A.M. Cloudy—light wind—rain early. P.M. Fine—light clouds and breeze. Evening, Overcast—heavy rain.
	T 23	30.076	30.070	64.9	30.038	30.032	67.6	60	04.8	63.3	67.5	58.0	69.8	.283	S	Overcast—high wind, with occasional rain throughout the day. Evening, The like.
	W24	29.818	29.810	65.7	29.790	29.786	68.0	60	05.2	63.8	67.2	61.2	69.3	.186	S	A.M. Overcast—brisk wind. P.M. Lightly cloudy—light rain and wind. Evening, Fine and clear.
	T25	29.800	29.794	64.8	29.806	29.800	67.7	60	05.5	63.0	69.8	58.8	69.3		S	A.M. Overcast—light wind. P.M. Fine—lt. clouds & wind. Ev. The like.
●	F26	29.794	29.788	65.3	29.680	29.672	65.3	58	06.5	62.3	60.2	55.6	70.3		S	A.M. Cloudy—light rain and wind. P.M. Overcast—light steady rain the rest of the day.
	S 27	29.620	29.614	69.3	29.658	29.652	65.9	59	06.6	61.2	61.7	53.5	67.0	.658	S	A.M. Cloudy—light brisk wind. P.M. Overcast—thunder and lightning, with rain. Evening, Overcast—heavy rain.
	⊙28	29.854	29.850	62.2	29.912	29.906	64.0	57	04.5	57.7	64.0	55.0	66.3	.383	WNW	Overcast—light breeze throughout the day. Evening, Cloudy.
	M29	30.068	30.062	64.2	30.008	30.000	64.3	56	06.9	62.3	61.8	53.2	65.6		S	A.M. Cloudy—light breeze. P.M. Overcast—light steady rain. Evening, Overcast—heavy rain—high wind.
	T30	29.804	29.798	61.8	29.768	29.762	62.9	56	04.6	57.7	61.8	54.9	65.8	.322	NW	A.M. Cloudy—light wind—high wind during the night. P.M. Overcast—very light rain. Evening, Light steady rain.
	W31	29.370	29.366	61.6	29.408	29.400	62.2	58	03.5	57.7	57.7	56.0	59.6	.261	S	Overcast—showery nearly the whole day, with high wind.
	MEAN.	29.940	29.934	68.2	29.920	29.914	67.0	58.0	06.7	63.1	67.3	56.7	69.9	Sum. 2.577	Mean Barometer corrected	9 A.M. 3 P.M. F. 29.830 .. 29.821 C. 29.831 .. 29.814
AUGUST	T 1	29.974	29.970	68.6	30.036	30.030	63.7	57	07.1	60.6	68.4	54.4	66.4	.215	W	A.M. Cloudy—lt. breeze. P.M. Fine—lt. clouds. Ev. Fine & clear.
	F 2	30.120	30.116	80.0	30.074	30.066	66.5	59	08.2	65.3	73.8	56.2	76.2		S	Fine & cloudless—light breeze throughout the day. Ev. Light fog.
	S 3	30.096	30.092	74.2	30.104	30.098	69.5	61	08.8	68.8	78.3	59.9	74.4		NNW	Fine and cloudless throughout the day. Evening, Fine—light clouds.
	⊙ 4	30.222	30.216	66.6	30.246	30.242	70.0	61	05.6	62.0	70.4	61.3	78.6		NNW	(Fine—light clouds and breeze throughout the day. Evening, Fine and clear.
	M 5	30.348	30.344	74.4	30.300	30.294	68.4	59	08*8	61.8	68.8	52.6	71.0		NW	(Fine and cloudless—light breeze throughout the day. Evening, Fine and clear.
	T 6	30.228	30.222	77.0	30.156	30.148	69.8	60	08.3	65.5	70.4	56.4	71.3		S	A.M. Fine—lt. clouds & breeze. P.M. Cloudy. Ev. Fine & clear.
	W 7	29.844	29.838	65.6	29.766	29.760	66.8	59	06.9	63.4	63.8	60.0	71.7		SSW	A.M. Cloudy—lt. breeze. P.M. Cloudy—occasional rain. Ev. Overcast—¼ p. 10 heavy rain, with thunder & lightning, for 15 min.
	T 8	29.998	29.992	74.4	30.050	30.044	68.3	60	07.5	61.2	65.7	53.7	71.2	.288	NW	Fine—light clouds and breeze throughout the day. Ev. Fine & clear.
○	F 9	30.188	30.182	77.6	30.136	30.130	68.3	59	07.1	64.0	69.3	53.9	76.3		SSW	Fine—light clouds and breeze throughout the day. Evening, Cloudy.
	S 10	30.152	30.146	76.9	30.082	30.076	68.3	60	06.0	64.6	69.7	56.2	71.3		SW	A.M. Cloudy—light breeze. P.M. Fine—light clouds. Evening, Fine and clear.
	⊙ 11	30.060	30.052	71.0	30.104	30.098	66.9	59	08.2	63.7	65.8	55.9	70.2		SW var.	A.M. Cloudy—brisk wind. P.M. Fine—light clouds and wind—(rainbow). Evening, Fine and clear.
	M12	30.314	30.306	68.7	30.280	30.276	61.8	57	06.6	59.3	63.6	53.2	67.0		NW	Cloudy—light breeze throughout the day. Evening, The same.
	T13	30.208	30.200	66.8	30.144	30.136	64.6	50	07.4	58.3	64.8	53.3	62.6		SE	A.M. Fine—lt. clouds & breeze. P.M. Cloudy—lt. wind. Ev. The same.
	W14	30.014	30.006	70.2	29.930	29.926	66.7	58	07.8	63.8	66.8	57.3	70.0		SE	Cloudy—light breeze throughout the day. Evening, The same.
	T15	29.696	29.690	62.7	29.626	29.622	64.2	55	05.7	61.0	63.8	57.8	68.7		E	A.M. Light fog and wind. P.M. Cloudy—light showers. Evening, Fine and clear.
	F16	29.654	29.650	71.2	29.634	29.630	67.0	59	06.3	64.3	68.7	56.9	65.0	.019	S	A.M. Fine—light clouds and breeze, with light showers. P.M. Fine —light clouds and wind. Evening, Cloudy.
	S 17	29.670	29.666	64.5	29.712	29.704	66.2	59	06.0	61.8	65.3	56.4	70.0	.686	S	A.M. ¼ p. 1 very heavy rain, which continued an hour and a half. P.M. Cloudy—light breeze. Evening, Overcast.
○	18	30.030	30.024	70.5	30.040	30.032	65.3	57	06.2	69.3	64.3	52.9	67.8		N	A.M. Fine—lt. clouds & breeze. P.M. Thick haze. Ev. Ovct.—heavy rain.
	M19	29.946	29.940	70.4	29.990	29.982	64.3	58	05.7	60.2	56.5	55.8	66.0	.347	WNW	(Cloudy—light wind, with showers nearly the whole of the day. Evening, The same.
	T20	30.080	30.072	60.2	30.110	30.102	62.3	51	03.8	52.5	55.4	49.4	59.4	.133	W	Overcast—light wind, with showers nearly the whole of the day. Evening, Fine and clear.
	W21	30.244	30.240	69.8	30.206	30.198	60.6	50	06.4	54.2	59.2	45.4	56.3	.025	NW	Fine—lt. clouds & wind throughout the day. Ev. Cloudy—light fog.
	T22	30.230	30.222	62.4	30.204	30.200	61.0	49	06.6	57.7	64.8	49.7	60.2		S	Fine—light clouds throughout the day. Evening, Fine and clear.
	F23	30.266	30.258	60.0	30.246	30.238	62.8	56	05.5	62.4	70.3	57.2	65.3		S	A.M. Light fog & wind. P.M. Fine—lt. clouds. Ev. Fine and clear.
●	S 24	30.140	30.132	63.8	30.062	30.056	65.4	59	04.9	62.5	69.5	57.6	70.8		S	A.M. Lightly overcast—lt. wind. P.M. Cloudy—lt. wind. Ev. The same.
⊙	25	30.026	30.018	64.2	29.972	29.968	66.5	59	05.4	62.3	70.6	60.0	70.5		W	A.M. Lightly overcast. P.M. Fine—light clouds. Ev. Fine & clear.
	M26	29.966	29.960	70.3	29.912	29.904	67.3	59	07.8	65.3	68.7	54.8	71.5		W	A.M. Fine—lt. clouds & wind. P.M. Cloudy—lt. wind. Ev. The same.
	T27	29.818	29.812	65.3	29.826	29.820	66.0	58	05.2	68.8	64.7	55.8	70.3		NW	(A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy—light rain.
	W28	30.072	30.064	61.2	30.048	30.042	63.3	54	04.6	55.4	65.3	50.9	65.0	.066	SW	A.M. Overcast—lt. wind. P.M. Fine—lt. clouds & wind. Ev. Cloudy.
	T29	29.942	29.936	62.3	29.892	29.884	65.3	60	04.1	62.3	67.7	54.9	65.3		S	A.M. Ovct.—brisk wind. P.M. Fine—lt. clds.—brisk wind. Ev. Cldy.
	F30	29.846	29.840	63.4	29.810	29.806	66.0	59	04.4	61.7	66.5	59.9	68.3	.133	SE	(Overcast—light wind, with occasional showers throughout the day. Evening, The same.
	S 31	29.530	29.526	63.0	29.382	29.378	63.5	61	03.3	61.8	59.3	57.0	66.8	.058	SSW	(Overcast—brisk wind, with occasional showers throughout the day. Evening, The same.
	MEAN.	30.030	30.024	68.3	30.003	29.996	65.7	57.5	06.3	62.1	66.5	55.4	68.6	Sum. 1.968	Mean Barometer corrected	9 A.M. 3 P.M. F. 29.906 .. 29.906 C. 29.919 .. 29.899

METEOROLOGICAL JOURNAL FOR SEPTEMBER AND OCTOBER, 1839.

1839.		9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M. Aug. Psychr.	Diff. of Wet and Dry Bulb Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	Self-registering. Lowest	Self-registering. Highest	Rain in inches at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
SEPTEMBER	⊙ 1	29.072	29.068	67.5	29.098	29.094	64.0	60	04.5	62.0	63.7	56.6	63.5	.180	SW var.	A.M. Overcast—showery—brisk wind. P.M. Fine—light clouds—high wind. Evening, The like. (Er. The like.
	M 2	29.220	29.216	67.0	29.204	29.200	63.0	53	06.8	59.8	61.0	53.0	64.8	.008	WSW	A.M. Fine—lt. clouds—brisk wind. P.M. Ovct.—showery—brisk wind.
	T 3	29.450	29.446	62.2	29.494	29.488	61.0	55	04.0	57.7	60.0	52.0	63.5	.122	SW var.	A.M. Overcast—brisk wind, with occasional rain. P.M. Fine—light clouds—brisk wind. Evening, The like—Aurora visible.
	W 4	29.764	29.760	61.0	29.856	29.852	61.0	54	04.5	57.5	65.0	50.0	61.0	.041	W	Fine—light clouds and wind throughout the day. Evening, Overcast—light rain throughout the night.
	T 5	29.918	29.912	60.8	29.894	29.890	62.5	58	03.2	62.4	66.0	57.0	66.0	.133	W	Overcast—light wind throughout the day. Evening, The like.
	F 6	30.032	30.028	66.2	30.040	30.036	63.2	58	04.0	61.8	67.0	56.0	67.0	.061	W	Fine—light clouds and wind throughout the day. Evening, The like.
	○ S 7	29.916	29.910	63.2	29.854	29.850	64.0	57	04.2	62.5	64.0	55.0	68.0	.011	S	Slightly overcast—brisk wind throughout the day. Er. The like.
	⊙ 8	29.978	29.974	66.0	30.032	30.028	64.8	56	05.7	60.8	66.2	56.2	65.7	.030	SW var.	Overcast—brisk wind throughout the day. Evening, Fine—light clouds—brisk wind.
	M 9	30.046	30.042	63.5	29.994	29.990	65.5	60	05.0	65.0	67.2	61.0	68.0		SW	Fine—light clouds—brisk wind throughout the day. Er. The like.
	T 10	30.030	30.026	63.0	30.100	30.094	65.0	60	02.0	62.4	69.3	57.0	69.5		W	A.M. Overcast—P.M. Fine—light clouds. Evening, Overcast.
	W 11	30.048	30.042	64.0	29.950	29.946	66.0	62	01.7	62.2	68.3	60.5	70.0		SE	Overcast—light fog throughout the day. Evening, The like.
	T 12	29.804	29.800	64.0	29.772	29.768	65.0	59	03.5	61.0	61.5	60.0	69.8	.019	NW	Overcast throughout the day. Evening, The like.
	F 13	29.672	29.668	64.2	29.572	29.568	63.0	54	04.5	57.5	60.4	49.0	63.0		SW	A.M. Fine—light clouds. P.M. Showery. Er. Lightly overcast.
	S 14	29.166	29.162	60.0	29.110	29.106	60.0	59	03.0	58.6	58.8	53.5	63.5	.013	SE	Overcast—light wind, with occasional showers throughout the day. Evening, Overcast—light rain.
	⊙ 15	29.148	29.144	60.0	29.198	29.192	61.6	58	02.5	58.5	61.2	55.0	60.9	.205	S	Overcast—light showers throughout the day. Evening, The like.
	M 16	29.368	29.364	61.5	29.382	29.378	61.4	55	03.5	58.5	61.5	53.5	62.5	.105	S	Overcast—brisk wind throughout the day. Evening, Overcast—rain—brisk wind.
	T 17	29.554	29.550	62.5	29.494	29.490	60.2	55	04.5	58.2	56.0	53.6	62.5	.102	SW	A.M. Overcast—brisk wind. P.M. Ovct.—steady rain. Er. The like.
	W 18	29.530	29.526	62.0	29.564	29.560	59.0	55	03.8	58.0	57.8	49.0	60.8	.691	SW	Fine—light clouds and wind throughout the day, with occasional showers. Evening, The same.
	T 19	29.518	29.514	56.4	29.488	29.484	58.0	53	03.8	58.5	56.7	51.0	60.0	.672	SSW	A.M. Lightly overcast—heavy rain early. P.M. Overcast—light rain and wind. Evening, Fine and clear.
	F 20	29.626	29.622	57.6	29.634	29.630	58.0	53	03.5	58.3	61.0	51.0	60.9	.036	SSW	A.M. Fine—lt. clouds & wind. P.M. Overcast—lt. wind. Ev. Lt. rain.
	S 21	29.528	29.532	57.0	29.554	29.550	58.4	52	01.4	51.7	56.3	51.9	61.7	.102	NW	A.M. Overcast—light rain and wind. P.M. Fine—light clouds and wind. Evening, Fine—light clouds.
	⊙ 22	29.628	29.622	57.3	29.624	29.618	58.8	52	03.1	55.4	58.7	50.0	58.7	.336	SSE	Overcast—light wind throughout the day. Ev. Overcast—light rain.
	● M 23	29.784	29.776	56.2	29.806	29.800	57.5	50	04.5	52.5	60.5	46.0	60.3	.044	SW	A.M. Fine—light fog & wind. P.M. Overcast. Er. Fine and clear.
	T 24	29.894	29.884	55.0	29.846	29.838	58.2	50	04.3	54.6	60.5	49.0	59.2		S	A.M. Light fog. P.M. Overcast—light wind. Evening, The same.
	W 25	29.768	29.762	58.2	29.796	29.792	60.0	57	02.7	60.3	63.0	54.0	60.0	.055	SSW	A.M. Overcast—brisk wind. P.M. Fine—light clouds—brisk wind. Evening, Overcast—brisk wind.
	T 26	29.856	29.850	60.2	29.844	29.840	61.0	55	04.0	59.0	61.9	55.0	64.5	.016	WSW	A.M. Lightly overcast—light wind. P.M. Fine—light clouds—brisk wind. Evening, The same.
	F 27	29.930	29.924	59.0	29.828	29.824	59.0	52	03.5	55.0	58.7	48.5	63.0		SSW	Lightly overcast—brisk wind throughout the day. Ev. The same.
	S 28	29.498	29.492	57.0	29.482	29.478	58.8	55	02.5	56.0	59.3	54.0	60.5		S	A.M. Overcast—light wind. P.M. Fine—light clouds. Ev. The same.
	⊙ 29	29.642	29.638	57.0	29.686	29.682	57.8	50	03.0	52.6	61.3	48.0	60.4		WSW	A.M. Lightly overcast—light wind. P.M. Fine—light clouds and wind. Evening, The same.
	M 30	29.856	29.850	54.4	29.884	29.880	56.3	49	03.0	51.5	59.5	45.0	60.0		E	A.M. Light fog. P.M. Lightly overcast. Evening, Cloudy.
MEAN.		29.675	29.670	60.8	29.669	29.665	61.1	55.2	03.7	58.3	61.7	53.0	63.5	Sum. 2.982	Mean Barometer corrected.....	9 A.M. 29.587 / 3 P.M. 29.593 .. 29.592 / Cr. 29.587 .. 29.592
OCTOBER	T 1	30.000	29.992	54.6	29.908	29.900	56.9	51	03.2	53.4	61.3	48.4	58.4		ENE	A.M. Fine—light fog. P.M. Fine—nearly cloudless—light breeze. Evening, Light fog.
	W 2	29.688	29.682	56.2	29.674	29.668	58.5	51	04.5	55.8	61.4	50.9	61.8		S	A.M. Fine—light clouds—fog early. P.M. Lightly cloudy. Evening, Cloudy—light rain.
	T 3	30.004	29.996	54.3	29.912	29.904	55.3	44	04.0	48.7	55.4	44.2	63.0	.102	SSW	A.M. Light fog. P.M. Fine—light clouds and wind. Evening, Overcast—light rain.
	F 4	29.456	29.450	54.8	29.486	29.480	57.0	51	03.7	54.8	58.0	47.9	55.3	.172	S	A.M. Overcast—lt. rain. P.M. Heavy shower. Ev. Continued rain.
	S 5	30.018	30.012	54.3	30.136	30.128	56.6	49	02.9	49.5	53.5	49.3	60.0	.538	NW	A.M. Overcast—brisk wind, as also during the night. P.M. Cloudy—light wind. Evening, Fine and clear.
	⊙ 6	30.302	30.298	51.9	30.284	30.280	54.0	47	03.3	49.8	56.6	44.7	52.3		N	A.M. Fine—light clouds and wind. P.M. Lightly overcast—light wind. Evening, Fine and clear.
	○ M 7	30.298	30.292	51.6	30.260	30.252	54.0	46	04.1	51.3	56.2	45.9	52.0		N	Fine—lt. clouds & wind throughout the day. Ev. Fine and clear.
	T 8	30.120	30.114	52.9	30.042	30.036	55.2	47	04.0	52.8	58.8	51.0	53.3		NW	Overcast—light fog and wind throughout the day. Ev. The like.
	W 9	29.876	29.868	57.3	29.840	29.832	60.0	55	03.7	59.7	62.2	51.4	60.0		S	Cloudy—lt. wind throughout the day. Er. Overcast—rain and wind.
	T 10	29.642	29.636	59.9	29.672	29.666	61.3	57	04.2	60.6	60.5	58.2	60.8	.027	S	Fine—light clouds—high wind throughout the day. Evening, Cloudy—light shower.
	F 11	29.598	29.594	59.2	29.590	29.584	61.6	57	04.0	59.7	66.6	54.7	62.3	.036	E	A.M. Fine and cloudless—light wind. P.M. Light clouds & wind. Evening, Cloudy—light rain.
	S 12	29.844	29.840	59.9	29.968	29.960	60.0	53	05.4	56.3	63.8	54.3	63.5	.250	S	A.M. Fine—light clouds—brisk wind—rain during the night. P.M. Fine—light clouds. Evening, Light fog.
	⊙ 13	30.020	30.014	56.7	29.996	29.992	58.0	51	03.3	52.3	57.2	49.6	52.4		SE	A.M. Thick fog. P.M. Fine—lt. clds.—brisk wind. Ev. Fine & clear.
	M 14	29.938	29.932	54.9	29.868	29.862	57.8	52	02.9	54.8	57.8	48.0	55.0		SE	Fine—nearly cloudless—lt. wind throughout the day. Ev. Fine & clear.
	T 15	29.818	29.812	54.5	29.874	29.868	55.9	49	02.6	52.2	54.4	50.2	59.0		S	A.M. Overcast—light mist—cloudy—light wind. Ev. Fine and clear.
	W 16	30.074	30.066	53.3	30.040	30.032	55.2	48	05.0	52.7	55.7	46.2	52.5	.033	SW	A.M. Fine and cloudless—rain during the night. P.M. Fine—light clouds. Evening, Fine and clear.
	T 17	30.092	30.086	51.6	30.076	30.068	53.7	46	03.1	48.0	56.4	43.8	51.3		S	A.M. Fine—light fog. P.M. Fine—light clouds. Evening, Overcast.
	F 18	29.866	29.858	52.8	29.876	29.870	54.3	50	01.4	50.8	52.5	47.5	54.7	.219	E	A.M. Overcast—light and wind—heavy rain during the night. P.M. Fine—light clouds—brisk wind. Ev. Overcast—lt. rain.
	S 19	30.044	30.036	51.9	30.066	30.058	53.3	48	03.5	50.2	54.7	47.0	51.6	.019	SW	A.M. Overcast—light fog and wind. P.M. Fine—light clouds. Evening, Light fog.
	⊙ 20	30.092	30.086	49.4	30.016	30.010	52.0	43	01.6	41.2	55.0	49.6	55.6		W	A.M. Thick fog—light wind. P.M. Fine—light clouds and wind. Evening, Light fog.
	M 21	30.064	30.056	52.6	30.072	30.066	53.9	49	02.3	54.0	56.2	41.0	54.3		ENE	Overcast—light wind throughout the day. Evening, Light fog.
	● T 22	30.156	30.148	54.2	30.130	30.124	54.7	50	02.9	53.3	54.7	52.8	53.6		SE	Overcast—lt. wind throughout the day. Ev. Overcast—very fine rain.
	W 23	30.086	30.078	54.3	30.060	30.052	54.8	49	03.2	53.2	53.2	52.7	53.4		SE	Overcast—light wind throughout the day, with occasional rain. Evening, The like.
	T 24	30.050	30.042	53.3	30.048	30.042	53.7	49	01.4	48.8	48.8	49.3	49.5	.175	E	Overcast—light rain and wind throughout the day. Er. The like.
	F 25	30.264	30.256	51.3	30.316	30.308	52.2	47	02.3	48.3	48.0	46.4	50.0	.266	NNE	A.M. Overcast—very light rain—brisk wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
	S 26	30.330	30.322	50.0	30.302	30.294	51.2	45	03.0	46.3	49.2	44.0	50.4		N	Fine—light clouds & wind throughout the day. Ev. Fine and clear.
	⊙ 27	30.272	30.268	47.5	30.218	30.212	49.0	42	02.4	42.0	47.0	40.6	50.0		NW	A.M. Fine and cloudless—light wind. P.M. Fine—light clouds and wind. Evening, Overcast—very fine rain and wind.
	M 28	30.314	30.306	45.3	30.314	30.306	47.2	39	02.9	42.7	45.7	40.4	49.0		N	A.M. Fine—nearly cloudless—light wind. P.M. Fine—light clouds and wind. Evening, Overcast—light rain.
	T 29	30.316	30.308	43.6	30.260	30.254	44.8	37	06.2	42.7	43.8	37.3	47.2	.060	NE	A.M. Cloudy—brisk wind. P.M. Fine—lt. clouds & wind. Ev. Overcast.
	W 30	30.142	30.134	43.2	30.052	30.046	45.2	38	03.6	41.4	45.8	39.8	45.0		NE	Cloudy—lt. brisk wind throughout the day. Ev. Overcast—lt. rain.
	T 31	29.940	29.932	42.8	29.876	29.870	42.7	38	01.7	39.3	40.7	39.8	44.8	.061	NE	Overcast—lt. rain—brisk wind throughout the day. Er. The like.
MEAN.		30.023	30.017	52.6	30.008	30.001	54.2	47.7	03.3	50.5	54.6	47.3	54.3	Sum. 1.958	Mean Barometer corrected	9 A.M. 29.962 .. 29.949 / 3 P.M. 29.962 .. 29.949 / Cr. 29.955 .. 29.936

The observations for the month of September were not taken by the Assistant Secretary, on account of absence.

METEOROLOGICAL JOURNAL FOR NOVEMBER AND DECEMBER, 1839.

1839.		9 o'clock, A.M. Barometer uncorrected. Flint Glass.	Crown Glass.	Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	Crown Glass.	Att. Ther.	Dew Points at 9 A.M. and 3 P.M.	Diff. of Wet and Dry Bulb Ther.	External Thermometers. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest	Highest	Rain in inches. Hour 9 A.M. to 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
NOVEMBER	F 1	29.714	29.706	42.6	29.672	29.664	43.8	39	02.3	41.2	44.8	39.8	41.7	.044	E	Overcast—high wind throughout the day, as also the evening.
	S 2	29.618	29.612	45.0	29.544	29.538	45.3	41	00.2	42.7	44.2	40.0	46.0	.044	N	Overcast—light rain—brisk wind throughout the day. Ev. The like.
	⊙ 3	29.612	29.604	46.6	29.556	29.550	48.2	41	01.2	48.2	51.2	42.7	49.0	.302	NE	{A.M. Light fog and wind—deposition. P.M. Overcast—it. wind. Evening, Rain and wind.
	M 4	29.512	29.504	49.8	29.538	29.530	51.2	46	02.2	50.3	52.3	46.2	51.7	.263	W	{A.M. Overcast—light wind. P.M. Fine—light clouds. Evening, Overcast—light rain.
	T 5	29.572	29.566	49.3	29.658	29.650	50.3	45	02.0	47.7	50.3	45.2	53.3	.088	W	{A.M. Lightly overcast—light rain and wind. P.M. Fine—light clouds. Evening, Overcast.
	○ W 6	29.746	29.742	48.8	29.712	29.704	49.2	46	02.3	45.7	48.0	45.3	46.5		W	A.M. Thick fog. P.M. Fine—light haze. Ev. Overcast—fine rain.
	T 7	29.624	29.618	49.2	29.616	29.608	50.3	46	01.3	48.3	50.8	45.6	49.2	.116	SE	Overcast—it. rain and wind throughout the day, as also the evening.
	F 8	29.806	29.800	51.2	29.684	29.676	52.2	48	02.2	49.8	52.2	48.5	52.0	.033	SE	{Overcast—light wind, with occasional fine rain throughout the day. Evening, Very fine rain.
	S 9	29.506	29.500	50.9	29.456	29.450	52.0	46	02.5	49.3	53.3	48.7	49.8		E	{A.M. Fine clouds and wind. P.M. Overcast—light wind. Evening, Very light rain.
	⊙ 10	29.378	29.372	52.3	29.206	29.200	52.8	49	01.6	52.4	51.3	48.7	54.6		E	Overcast—very light rain & wind throughout the day. Ev. The like.
	M 11	29.248	29.242	52.8	29.262	29.258	53.3	49	02.0	48.7	51.7	47.5	48.4	.463	S	Fine—light clouds and wind throughout the day. Ev. Overcast.
	T 12	29.418	29.412	51.3	29.466	29.458	52.5	48	01.8	47.9	53.2	46.0	48.7		SE	A.M. Light fog and wind. P.M. Fine—light clouds. Ev. Overcast.
	W 13	29.748	29.742	51.4	29.822	29.814	51.7	48	01.7	48.7	50.7	47.4	49.4		NNW	A.M. Light fog and wind. P.M. Overcast—it. wind. Ev. Light fog.
	T 14	29.938	29.980	51.2	29.978	29.970	51.5	48	02.4	47.4	52.3	47.4	48.0		SSE	{A.M. Fine—light clouds and wind. P.M. Overcast—light rain. Evening, Overcast—deposition.
	F 15	29.980	29.974	51.9	29.914	29.906	52.7	49	02.2	52.5	53.2	47.4	52.8		SE	Overcast—light fog throughout the day. Evening, Light rain.
	S 16	29.886	29.880	53.2	29.900	29.894	54.3	50	02.3	52.8	55.4	51.5	53.0	.036	SW	A.M. Light fog and wind. P.M. Fine—light clouds. Ev. Cloudy.
	○ 17	30.044	30.036	52.8	30.034	30.030	54.0	50	01.8	51.8	54.5	50.6	52.4		S	Overcast, with occasional rain throughout the day. Ev. The like.
	M 18	29.992	29.984	54.4	29.966	29.958	55.0	52	02.2	54.8	53.8	50.6	55.3	.150	S	{Overcast—light rain and wind throughout the day. Evening, Cloudy—light rain.
	T 19	29.992	29.986	53.3	29.920	29.914	54.0	50	01.9	49.4	52.8	47.9	50.3	.305	S	Fine—it. clouds & wind throughout the day. Ev. Overcast—it. rain.
	W 20	30.064	30.056	51.6	30.090	30.082	51.6	45	02.4	44.7	48.4	44.6	49.7	.063	W	{A.M. Lightly cloudy—light wind. P.M. Fine—light clouds. Evening, Fine and clear.
	● T 21	29.472	29.468	48.9	29.482	29.476	48.6	44	02.1	45.2	44.2	42.4	48.2	.244	E	{Overcast—brisk wind, with occasional rain throughout the day. Rain during the night. Evening, Fine and clear.
	F 22	29.916	29.908	46.6	30.032	30.024	47.3	42	01.7	42.2	45.0	39.6	42.7		NW	A.M. Fine—light clouds and wind. P.M. Cloudy. Ev. Fine & clear.
	S 23	30.208	30.200	43.2	30.220	30.212	44.2	39	01.5	38.2	43.6	35.7	40.5		NW	A.M. Light fog & wind. P.M. Fine—it. clouds & wind. Ev. Cloudy.
	⊙ 24	30.156	30.150	42.7	29.928	29.920	43.8	38	01.5	40.3	46.7	37.0	40.7		SW	{Overcast—light wind with occasional rain throughout the day. Evening, The like.
	M 25	29.680	29.674	47.3	29.542	29.534	49.0	45	02.4	50.7	55.5	40.3	51.2	.044	SW	Overcast—very fine rain throughout the day. Evening, The like.
	T 26	29.420	29.412	47.9	29.386	29.378	48.0	45	01.5	42.2	43.5	38.2	42.5	.169	SSW	{A.M. Light fog and wind. P.M. Fine—light clouds. Evening, Fine and clear—frost during the night.
	W 27	29.436	29.430	42.8	29.424	29.416	42.3	37	01.6	34.7	35.7	34.6	44.6		S	{A.M. Thick fog. P.M. Overcast—light rain and snow. Evening, Continued heavy rain.
	T 28	29.546	29.540	41.7	29.552	29.548	42.3	37	00.8	39.2	43.3	33.4	39.8	1.460*	E	{A.M. Overcast—light rain and wind—heavy rain throughout the night. P.M. Overcast—brisk wind. Evening, The like.
	F 29	29.268	29.260	43.4	29.170	29.166	44.8	39	00.2	43.3	44.7	39.4	44.0	.133	E	Overcast—light rain—brisk wind throughout the day. Ev. The like.
	S 30	29.606	29.598	45.2	29.638	29.632	46.0	38	02.0	42.4	46.4	42.3	42.7	.250	S	{Fine—nearly cloudless—light wind throughout the day. Evening, Overcast—very light rain.
MEAN.		29.705	29.699	48.6	29.679	29.672	49.4	44.7	01.8	46.4	49.1	43.8	48.0	Sum. 4.207	Mean Barometer corrected	9 A.M. 3 P.M. F. 29.636 .. 29.627 C. 29.649 .. 29.619
DECEMBER	⊙ 1	29.708	29.700	43.4	29.734	29.726	43.2	38	01.1	37.7	39.8	37.6	38.4		E	Thick fog throughout the day, as also the evening.
	M 2	29.882	29.874	41.9	29.892	29.886	42.3	36	01.5	38.8	42.3	37.2	39.4		E	A.M. Thick fog. P.M. Overcast—deposition. Evening, Light fog.
	T 3	29.960	29.952	42.9	29.948	29.940	42.2	38	01.9	38.8	39.4	35.8	39.4		S	Light fog throughout the day, as also the evening.
	W 4	29.830	29.822	39.8	29.800	29.794	40.2	34	02.4	34.3	37.8	32.0	34.6	.038	NNW	Fine—it. clouds & wind throughout the day. Evening, Fine & clear.
	T 5	30.096	30.090	39.0	30.152	30.146	39.7	33	01.8	36.7	37.3	33.3	37.2		SW	{A.M. Light fog and wind. P.M. Light clouds and wind. Evening, Cloudy—light rain.
	○ F 6	30.364	30.360	40.3	30.356	30.350	40.2	35	01.2	36.7	36.2	36.0	37.3		W	A.M. Thick fog—deposition. P.M. Ovcst.—deposition. Ev. The like.
	S 7	30.332	30.324	39.3	30.278	30.270	40.5	33	01.1	34.9	39.7	34.2	35.3		E	A.M. Light fog—deposition. P.M. Fine—light clouds. Evening, Fine and clear—frosty.
	⊙ 8	30.096	30.090	37.6	30.018	30.012	37.8	32	01.1	35.4	35.7	33.4	35.9		E	Overcast—light brisk wind throughout the day, as also the evening.
	M 9	29.830	29.824	37.3	29.730	29.724	37.6	31	01.8	34.9	36.3	34.7	36.0		NW	Overcast—deposition throughout the day, as also the evening.
	T 10	29.606	29.600	37.4	29.586	29.580	37.8	31	02.3	34.2	36.0	34.2	36.7		E	{Overcast—light wind—frosty throughout the day. Evening, Overcast—deposition.
	W 11	29.664	29.656	39.0	29.386	29.382	40.4	37	01.3	42.4	43.7	34.3	43.0		E	{Overcast—deposition—light wind throughout the day. Evening, Overcast—light rain and wind.
	T 12	29.248	29.242	42.0	29.142	29.138	43.6	39	02.1	44.8	46.8	41.9	45.3	.083	E	{A.M. Overcast—brisk wind. P.M. Fine—light clouds. Evening, Overcast—light rain.
	F 13	29.294	29.288	43.8	29.268	29.262	44.6	40	02.2	43.3	46.3	41.3	46.4	.061	S	A.M. Fine—it. clouds and wind. P.M. Cloudy, as also the evening.
	S 14	29.230	29.226	44.7	29.296	29.290	45.7	39	01.9	44.2	46.3	42.3	44.4	.250	S	{A.M. Overcast—brisk wind—heavy rain during the night. P.M. Fine—light clouds—showery. Evening, Fine and clear.
	⊙ 15	29.500	29.584	43.2	29.496	29.492	44.0	36	01.3	39.3	43.2	38.8	40.0		SW	A.M. Light fog—deposition. P.M. Overcast. Evening, Light rain.
	M 16	29.696	29.690	43.2	29.846	29.840	43.7	38	00.5	40.3	42.5	38.8	40.5	.122	W	Overcast—light wind throughout the day. Evening, Fine & clear.
	T 17	30.074	30.068	42.5	29.964	29.956	43.2	37	01.5	38.3	41.5	37.4	38.8		NE	Fine—light clouds and wind throughout the day. Evening, Cloudy.
	W 18	29.516	29.508	39.9	29.400	29.394	41.6	32	02.0	35.2	38.7	34.7	35.5		E	{A.M. Cloudy—light brisk wind. P.M. Overcast—light rain and wind. Evening, The like.
	T 19	29.418	29.412	42.6	29.412	29.404	44.7	40	01.1	47.7	49.7	34.7	48.4	.333	S	{Overcast—light wind, with occasional rain throughout the day. Evening, The like.
	● F 20	29.282	29.276	48.7	29.276	29.272	50.0	46	01.1	51.3	52.8	47.3	51.7	.322	S	{A.M. Overcast—light rain and wind. P.M. Cloudy—light wind. Evening, Overcast—light rain.
	S 21	29.552	29.548	49.9	29.532	29.528	50.9	45	01.5	47.3	49.9	46.6	47.7	.180	S	{Overcast—very fine rain throughout the day. Ev. Overcast—light rain.
	⊙ 22	29.550	29.544	53.3	29.462	29.458	52.0	49	00.7	49.9	52.5	46.8	50.6	.061	SW	{Overcast—very fine rain throughout the day, as also the evening, with high wind.
	M 23	29.586	29.580	51.8	29.484	32.476	52.8	48	01.5	48.6	49.8	48.4	49.3	.125	S	{A.M. Fine—light clouds and wind. P.M. Overcast—light rain. Evening, Overcast—light rain—high wind.
	T 24	29.244	29.238	53.8	29.306	29.300	54.2	52	02.5	53.4	50.5	48.5	54.6	.288	S	{A.M. Dark heavy clouds—high wind—very high wind throughout the night. P.M. Light clouds. Ev. Overcast—brisk wind.
	W 25	29.528	29.520	50.7	29.562	29.554	51.0	48	02.3	47.7	46.5	45.9	49.4		S	Fine—it. clouds & wind throughout the day. Ev. Fine starlight night.
	T 26	29.732	29.726	45.9	29.500	29.496	45.2	42	01.6	40.7	40.7	38.3	41.2		S	A.M. Overcast—it. fog & wind. P.M. Light rain & wind. Ev. The like.
	F 27	29.342	29.334	47.7	29.488	29.480	48.0	45	01.6	47.2	43.8	39.4	47.8	.305	W	{A.M. Overcast—light rain and wind. P.M. Fine—light clouds. Evening, Overcast—light rain.
	S 28	29.752	29.746	44.3	29.822	29.816	44.3	40	01.5	35.7	39.4	35.8	36.0	.158	NW	{A.M. Light fog—deposition. P.M. Overcast. Evening, Fine starlight night—sharp frost.
	⊙ 29	30.280	30.272	40.5	30.350	30.344	41.0	33	00.5	33.3	38.2	32.2	33.5		W	{Fine—light clouds throughout the day—white frost. Evening, Fine and clear—sharp frost.
	M 30	30.398	30.390	38.2	30.284	30.278	38.8	33	00.6	32.3	39.8	32.0	35.2		SW	A.M. Thick fog—sharp frost. P.M. Ovct.—it. wind. Ev. Very it. rain.
	T 31	29.924	29.916	41.0	29.798	29.792	43.3	38	01.3	44.8	47.6	32.0	45.3		S	A.M. Ovct.—it. wind. P.M. Fine—it. clouds. Ev. Ovct.—deposition.
MEAN.		29.729	29.723	43.4	29.696	29.690	44.0	38.6	01.5	41.0	43.9	38.4	41.8	Sum. 2.326	Mean Barometer corrected	9 A.M. 3 P.M. F. 29.693 .. 29.659 C. 29.686 .. 29.651

* An unusual quantity during the twenty-four hours.

The quantity of rain has been unusually great for the year 1839 compared with those for 1837 and 1838 :—

In. Parts.	In. Parts.
1837, 17.949	1838, 19.537 ..... and 1839, 24.504.